N.° 6

Philip Earl Stanhope.

PHILOSOPHICAL TRANSACTIONS,

GIVING SOME

ACCOUNT

OF THE

Present Undertakings, Studies, *and* Labours,

OF THE

INGENIOUS,

IN MANY

Considerable Parts of the WORLD.

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PHILOSOPHICAL TRANSACTIONS.

For the Month of *January*, 1747-8.

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A Letter to the Right honourable George *Earl of* Macclesfield *concerning an* apparent Motion *observed in some of the* fixed Stars; *by* James Bradley *D. D. Astronomer Royal, and F. R. S.*

My LORD,

Read at a Meeting of the Royal Society, Febr. 14. 1747.

THE great Exactness, with which Instruments are now constructed, hath enabled the Astronomers of the present Age to discover several Changes in the Positions of the heavenly Bodies; which, by reason of their *Smallness*, had escaped the Notice of their Predecessors. And altho' the Causes of such Motions have always subsisted, yet Philosophers had not so fully consider'd, what the Effects of those known Causes would be, as to demonstrate *a priori* the *Phænomena* they might produce; so that Theory itself is here, as well as in many other Cases, indebted to Practice, for the Discovery of some of its most elegant Deductions. This points out to us the great Advantage of cultivating *this*, as well as every other Branch of Natural Knowledge, by a regular Series of Observations and Experiments.

The Progress of Astronomy indeed has always been found, to have so great a Dependence upon

accurate Obſervations, that, till ſuch were made, it advanced but ſlowly: For the firſt conſiderable Improvements that it received, in point of Theory, were owing to the renowned *Tycho Brakè*; who far exceeding thoſe that had gone before him, in the Exactneſs of his Obſervations, enabled the ſagacious *Kepler* to find out ſome of the principal Laws, relating to the Motion of the heavenly Bodies. The Invention of Teleſcopes and Pendulum-Clocks affording proper Means of ſtill farther improving the *Praxis* of Aſtronomy; and theſe being alſo ſoon ſucceeded by the wonderful Diſcoveries made by our Great *Newton*, as to its Theory; the Science, in both reſpects, had acquired ſuch extraordinary Advancement, that future Ages ſeemed to have little room left, for making any great Improvements. But, in fact, we find the Caſe to be very different; for, as we advance in the means of making more nice Inquiries, new Points generally offer themſelves, that demand our Attention. The Subject of my preſent Letter to your Lordſhip, is a Proof of the Truth of this Remark: for, as ſoon as I had diſcovered the Cauſe, and ſettled the Laws of the Aberrations of the fixed Stars, ariſing from the Motion of Light, *&c.* whereof I gave an Account in N°. 406. of the *Philoſophical Tranſactions*; my Attention was again excited by another *new Phænomenon*, *viz.* an annual Change of Declination in ſome of the fixed Stars; which appeared to be ſenſibly *greater* about that time, than a Preceſſion of the Equinoctial Points of 50″ in a Year would have occaſioned. The Quantity of the Difference, tho' ſmall in itſelf,

was

was rendered perceptible, thro' the Exactness of my Instrument, even in the first Year of my Observations; but being then at a Loss to guess, from what Cause that greater Change of Declination proceeded, I endeavoured to allow for it in my Computations, by making use of the *observed* annual Difference, as mentioned in *p.* 652. of the same *Transaction*.

From *that* time to the present, I have continued to make Observations at *Wansted*, as Opportunity offered, with a View of discovering the Laws and Cause of this *Phænomenon*: For, by the Favour of my very kind and worthy Friend *Matthew Wymondesold* Esq, my Instrument has remained, where it was first erected; so that I have been able, without any Interruption, which the Removal of it to another Place would have occasioned, to proceed on with my intended Series of Observations, for the Space of twenty Years: a Term somewhat exceeding the whole Period of the Changes, that happen in this *Phænomenon*.

When I shall mention the *small* Quantity of the Deviation, which the Stars are subject to, from the Cause that I have been so long searching after; I am apprehensive, that I may incur the Censure of some Persons, for having spent so much Time in the Pursuit of such a seeming Trifle: But the candid Lovers of Science will, I hope, make due Allowance for that natural Ardour, with which the Mind is urged on towards the Discovery of Truths, in themselves perhaps of *small* Moment, were it not that they tend to illustrate others of greater Use.

The apparent Motions of the heavenly Bodies are so complicated, and affected by such a Variety of

Cauſes; that in many Caſes it is extremely difficult to aſſign to each its due Share of Influence; or diſtinctly to point out, what Part of the Motion is the Effect of one Cauſe, and what of another: And whilſt the joint Effects of *All* are only attended to, great Irregularities and ſeeming Inconſiſtencies frequently occur; whereas, when we are able to allot to each particular Cauſe its proper Effect, Harmony and Uniformity uſually enſue.

Such ſeeming Irregularities being alſo blended with the unavoidable Errors, which Aſtronomical Obſervations muſt be always liable to, as well from the Imperfection of our Senſes, as of the Inſtruments that we make uſe of, have often very much perplex'd thoſe, who have attempted to ſolve the *Phænomena*: and till Means are diſcovered, whereby we can ſeparate and diſtinguiſh the *particular* Part of the whole Motion, that is owing to each reſpective Cauſe, it will be impoſſible, to be well aſſured of the Truth of any Solution. For theſe Reaſons, we generally find, that the more exact the Inſtruments are, that we make uſe of, and the more regular the Series of Obſervations is, that we take; the ſooner we are enabled to diſcover the Cauſe of any new *Phænomenon*. For when we can be well aſſured of the Limits, wherein the Errors of the Obſervations are contain'd; and have reduced them within as narrow Bounds as poſſible, by the Perfection of the Inſtruments which we employ; we need not heſitate to aſcribe ſuch apparent Changes, as manifeſtly exceed thoſe Limits, to ſome other Cauſes. Upon theſe Accounts it is incumbent upon the *practical*

Aſtronomer, to ſet out at firſt with the Examination of the Correctneſs of his Inſtruments; and to be aſſured that they are ſufficiently exact for the Uſe he intends to make of them: or at leaſt he ſhould know, within what Limits their Errors are confined.

This Practice has, in an eminent manner, been lately recommended by your Lordſhip's noble Example; who having, out of a ſingular Regard for the Science of Aſtronomy, erected an Obſervatory, and furniſhed it with as complete an *Apparatus* of Inſtruments, as our beſt Artiſts could contrive; would not fully rely on their Exactneſs, till their Diviſions had undergone the ſtricteſt Re-examination: whereby they are probably now render'd as perfect in their kind, as any extant, or as human Skill can at preſent produce.

The Lovers of *this* Science in general, cannot but acknowledge their Obligations to your Lordſhip on this Account; but I find myſelf more particularly bound to do it; ſince, by means of your Lordſhip's moſt accurate Obſervations, I have been enabled to ſettle ſome *principal Elements*; which I could not at preſent otherwiſe have done, for want of an Inſtrument at the Royal Obſervatory, *proper* for that Purpoſe: For the large *mural Quadrant*, which is there fixed to obſerve Objects lying Southward of the Zenith, however *perfect* an Inſtrument it may be in it ſelf, is not *alone* ſufficient to determine, with proper Exactneſs, either the *Latitude* of the Obſervatory, or the Quantity of Refraction correſponding to different Altitudes: For it being too heavy to be conveniently removed; and the Room wherein it is placed, being too ſmall to admit of its

Being

being turned to the opposite Side of the Wall, whereon it now hangs; I cannot, by *actual* Observations of the circumpolar Stars, settle those necessary Points; and therefore have endeavoured to do it, by comparing my own with your Lordship's Observations: and until this Defect in the *Apparatus* belonging to the Royal Observatory be removed, we must be indebted to your Lordship, for the Knowledge of its true Situation.

A Mind intent upon the Pursuit of any kind of Knowledge, will always be agreeably entertained, with what can supply the most proper means of attaining it: Such, to the practical Astronomer, are exact and well-contriv'd Instruments; And I reflect with Pleasure on the Opportunities I have enjoyed, of cultivating an Acquaintance and Friendship with the Person, that, of all others, has most contributed to their Improvement. For I am sensible, that if my own Endeavours have, in any respect, been effectual to the Advancement of Astronomy; it has principally been owing to the Advice and Assistance given me by our worthy Member Mr. *George Graham*; whose great Skill and Judgment in Mechanicks, join'd with a complete and practical Knowledge of the Uses of Astronomical Instruments, enable him to contrive and execute them in the most perfect manner.

The Gentlemen of the *Royal Academy* of *Sciences*, to whom we are so highly obliged for their exact Admeasurement of the Quantity of a Degree under the Arctic Circle, have already given the World very convincing Proofs of *his* Care and Abilities in those Respects; and the particular Delineation, which they have lately published, of the several Parts

of

of the *Sector*, which he made for them, hath now rendered it needleſs, to enter upon any minute Deſcription of *mine* at *Wanſted*; both being conſtructed upon the ſame Principles, and differing in their component Parts, chiefly on account of the different Purpoſes, for which they were intended.

As mine was originally deſigned to take only the *Differences* of the Zenith Diſtances of Stars, in the various Seaſons of the Year, without any View of diſcovering their *true* Places; I had no Occaſion to know exactly, what Point on the Limb correſponded to the *true* Zenith: and therefore no Proviſion was made in my Sector, for the changing of its Situation for that Purpoſe. Neither was it neceſſary that the Diviſions or Points on the Arc ſhould be ſet off, with the utmoſt Accuracy, Equidiſtant from each other; becauſe, when I obſerve any particular Star, the ſame Spot or Point being firſt biſected by the Plumb-line, and then the Screw of the Micrometer turn'd until the Star appears upon the middle of the Wire, that is fixed in the common Focus of the Glaſſes of the Teleſcope; I can thereby collect, how far the Star is from that given Point at the Time of Obſervation: and afterwards, by comparing together the ſeveral Obſervations that are made of it, I am able to diſcover what apparent Change has happen'd. The Quantity of the viſible Alteration, in the Poſition of the Stars, being expreſſed by Revolutions and Parts of a Revolution, of the Screw of the Micrometer; I endeavoured to determine, with great Care, the true Angle anſwering thereto: and after various Trials, I thoroughly ſatisfied myſelf, both of the Equality of

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the Threads of the Screw, and of the precise Number of Seconds corresponding to them.

But altho' these Points could be settled with great Certainty, I was nevertheless obliged to make one Supposition; which perhaps to some Persons may seem of too great Moment in the present Inquiry, to be admitted without an evident Proof from Facts and Experiments. For I *suppose*, that the Line of Collimation of my Telescope has invariably preserved the same Direction, with respect to the Divisions upon the Arc, during the whole Course of my Observations. And indeed it was on account of the Objections, which might have been raised against such a *Postulate*, that I thought it necessary, to continue my Series of Observations for so many Years, before I published the Conclusions, which I shall at present endeavour to draw from them.

Whoever compares the Result of the several Trials, that have been made by the Gentlemen of the *Academy* of *Sciences*, for determining the Zenith Point of their Sector, since their Return from the North; will, I presume, allow that *mine* is not an unreasonable or precarious *Supposition*: since it is evident, from their Observations, that the Line of Collimation of that Instrument underwent no sensible Change in its Direction, during the Space of more than a whole Year; altho' it was several times taken down, and set up again, in different and remote Places; whereas *mine* hath always remained suspended in the same Place.

But besides such a strong Argument for the Probability of the Truth of my *Supposition*, I have the Satisfaction of finding it *actually* verified by the

Observations

Obſervations themſelves; which plainly prove, that at the End of the full Period of the Deviations which I am going to mention, the Stars are found to have the ſame Poſitions by the Inſtrument, as they ought to *have*, ſuppoſing the Line of Collimation to have continued unaltered from the Time when I firſt began to obſerve.

I have already taken notice, in what manner this *Phænomenon* diſcover'd itſelf to me at the End of my firſt Year's Obſervations, *viz.* by a *greater* apparent Change of Declination in the Stars near the Equinoctial Colure, than could ariſe from a Preceſſion of 50″ in a Year; the mean Quantity now uſually allowed by Aſtronomers. But there appearing at the ſame time, an Effect of a quite contrary Nature, in ſome Stars near the Solſtitial Colure, which ſeem'd to alter their Declination *leſs* than a Preceſſion of 50″ required; I was thereby convinced, that all the *Phænomena*, in the different Stars, could not be accounted for, merely by ſuppoſing, that I had aſſumed a wrong Quantity for the Preceſſion of the Equinoctial Points.

At firſt, I had a Suſpicion, that ſome of theſe ſmall apparent Alterations in the Places of the Stars, might poſſibly be occaſioned by a Change, in the Materials, or in the Poſition of the Parts of my Sector: But, upon conſidering how firmly the Arc, on which the Diviſions or Points are made, is faſtened to the Plate, wherein the Wire is fixed that lies in the Focus of the Object-Glaſs; I ſaw no Reaſon to apprehend, that any Change could have happened in the Poſition of that Wire and thoſe Points. The Suſpenſion therefore of the Plummet being the moſt likely Cauſe, from whence I conceived any Uncer-

tainty could arise; and the Wire of which had been broken three or four times in the first Year of my Observations: I attempted to examine, whether Part of the 'forementioned apparent Motions might not have been owing, to the different Plumb-lines that had been made use of. In order to determine this, I adjusted a particular Point of the Arc to the Plumb-line, with all the Exactness I could; and then taking off the old Wire, I immediately hung on another, with which the same Spot was again compared. I repeated the Experiment three or four times, and thereby fully satisfied myself, that no sensible Error could arise from the Use of different Plumb-lines; since the various Adjustments of the same Point agreed with each other, within less than half a Second.

Having then, from such Trials, sufficient Reason to conclude, that these *second* unexpected Deviations of the Stars, were not owing to any Imperfection of my Instrument; after I had settled the Laws of the Aberrations arising from the Motion of Light, *&c.* I judged it proper to continue my Observations of the same Stars; hoping that, by a regular and longer Series of them, carried on thro' several succeeding Years, I might, at length, be enabled to discover the *real* Cause of such apparent Inconsistencies.

As I resided chiefly at *Wansted*, after my Sector was erected there in the Year 1727. till the Beginning of *May* 1732. when I removed from thence to *Oxford*: I had, during my Abode at *Wansted*, frequent Opportunities of repeating my Observations; and thereby discovered so many Particulars relating

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to these *Phænomena*, that I began to guess what was the real Cause of them.

It appeared from my Observations, that, during this Interval of Time, some of the Stars near the Solstitial Colure, had changed their Declinations 9″ or 10″ *less*, than a Precession of 50″ would have produced; and, at the same time, that, others near the Equinoctial Colure, had altered theirs about the same Quantity *more*, than a like Precession would have occasioned: the North Pole of the Equator seeming to have approached the Stars, which come to the Meridian with the Sun, about the Vernal Equinox and the Winter Solstice; and to have receded from those, which come to the Meridian with the Sun, about the Autumnal Equinox and the Summer Solstice.

When I consider'd these Circumstances, and the Situation of the Ascending Node of the Moon's Orbit, at the time when I first began my Observations; I suspected, that the Moon's Action upon the Equatorial Parts of the Earth might produce these Effects: For, if the Precession of the Equinox be, according to Sir *Isaac Newton*'s Principles, caused by the Actions of the Sun and Moon upon those Parts; the Plane of the Moon's Orbit being at *one* time, above ten Degrees *more* inclined to the Plane of the Equator, than at *another*; it was reasonable to conclude, that the Part of the whole annual Precession, which arises from her Action, would in different Years be varied in its Quantity; whereas the Plane of the Ecliptic, wherein the Sun appears, keeping always nearly the same Inclination to the Equator; *that* Part of the Precession, which is owing to the Sun's Action, may be the same every

 Year:

Year: And from hence it would follow, that, altho' the *mean* annual Preceſſion, proceeding from the joint Actions of the Sun and Moon, were 50″; yet the *apparent* annual Preceſſion might ſometimes exceed, and ſometimes fall ſhort, of that mean Quantity, according to the various Situations of the Nodes of the Moon's Orbit.

In the Year 1727. when my Inſtrument was firſt ſet up, the Moon's Aſcending Node was near the Beginning of *Aries*; and conſequently, her Orbit was as much inclined to the Equator, as it can at any time be; and then the *apparent* annual Preceſſion was found, by my firſt Year's Obſervations, to be greater than the *mean:* which proved, that the Stars near the Equinoctial Colure, whoſe Declinations are moſt of all affected by the Preceſſion, had changed *theirs*, above a tenth Part more than a Preceſſion of 50″ would have cauſed. The ſucceeding Years Obſervations proved the ſame Thing; and in three or four Years time the Difference became ſo conſiderable, as to leave no Room to ſuſpect, that it was owing to any Imperfection, either of the Inſtrument or Obſervations.

But ſome of the Stars, which I had obſerved, that were near the Solſtitial Colure, having appeared to move, during the ſame time, in a manner contrary to what they ought to have done, by an Increaſe in the Preceſſion; and the Deviations in them being as remarkable as in the others, I perceived that ſomething more, than a mere Change in the Quantity of the Preceſſion, would be requiſite to ſolve this Part of the *Phænomenon.* Upon comparing my Obſervations of Stars near the Solſtitial Colure, that were

almoſt oppoſite to each other in Right Aſcenſion, I found, that they were equally affected by this Cauſe; for whilſt *γ Draconis* appeared to have moved Northward, the ſmall Star, which is the 35th *Camelopardali Hevel.* in the *Britiſh* Catalogue, ſeem'd to have gone as much towards the South: which ſhew'd, that this apparent Motion, in both thoſe Stars, might proceed from a Nutation in the Earth's Axis; whereas the Compariſon of my Obſervations of the ſame Stars, *formerly* enabled me to draw a different Concluſion, with reſpect to the Cauſe of the annual Aberrations ariſing from the Motion of Light. For the apparent Alteration in *γ Draconis*, from *that* Cauſe, being as great again as in the other ſmall Star, proved, that *that Phænomenon* did not proceed from a *Nutation* of the Earth's Axis; as, on the contrary, *this* may. Upon making the like Compariſon between the Obſervations of other Stars, that lie nearly oppoſite in Right Aſcenſion, whatever their Situations were with reſpect to the Cardinal Points of the Equator, it appeared, that their Change of Declination was nearly equal, but contrary; and ſuch as a Nutation or Motion of the Earth's Axis would effect.

The Moon's Aſcending Node being got back towards the Beginning of *Capricorn* in the Year 1732. the Stars near the Equinoctial Colure appeared, about that time, to change their Declinations no more, than a Preceſſion of 50″ required; whilſt ſome of thoſe near the Solſtitial Colure altered *theirs* above 2″ in a Year leſs, than they ought. Soon after, I perceived the annual Change of Declination of the former to be diminiſhed, ſo as to become *leſs* than

50″

50″ of Preceſſion would cauſe; and it continued to diminiſh till the Year 1736. when the Moon's Aſcending Node was about the Beginning of *Libra*, and her Orbit had the *leaſt* Inclination to the Equator: But by this time, ſome of the Stars near the Solſtitial Colure had altered their Declinations 18″ *leſs*, ſince the Year 1727. than they ought to have done from a Preceſſion of 50″. For γ *Draconis*, which in thoſe nine Years ſhould have gone about 8″ more *Southerly*, was obſerved in 1736. to appear 10″ more *Northerly*, than it did in the Year 1727.

As this Appearance in γ *Draconis*, indicated a Diminution of the Inclination of the Earth's Axis to the Plane of the Ecliptic; and as ſeveral Aſtronomers have ſuppoſed *that* Inclination to diminiſh regularly; if this *Phænomenon* depended upon ſuch a Cauſe, and amounted to 18″ in nine Years, the Obliquity of the Ecliptic would, at that rate, alter a whole Minute in thirty Years; which is much faſter than any Obſervations, *before* made, would allow. I had Reaſon therefore to think, that *ſome Part* of this Motion at the leaſt, if not the *Whole*, was owing to the Moon's Action upon the Equatorial Parts of the Earth; which I conceived, might cauſe a libratory Motion of the Earth's Axis. But as I was unable to judge, from only nine Years Obſervations, whether the Axis would entirely recover the ſame Poſition, that it had in the Year 1727. I found it neceſſary to continue my Obſervations thro' a whole Period of the Moon's Nodes; at the End of which I had the Satisfaction to ſee, that the Stars returned into the ſame Poſitions again; as if there had been no Alteration at all in the Inclination of the Earth's Axis: which

which fully convinced me, that I had guessed rightly as to the Cause of the *Phænomena*. This Circumstance proves likewise, that if there be a gradual Diminution of the Obliquity of the Ecliptic; it does not arise only from an Alteration in the Position of the Earth's Axis, but rather from some Change in the Plane of the Ecliptic itself: because the Stars, at the End of the Period of the Moon's Nodes, appeared in the same Places, with respect to the Equator, as they ought to have done, if the Earth's Axis had retained the same Inclination to an invariable Plane.

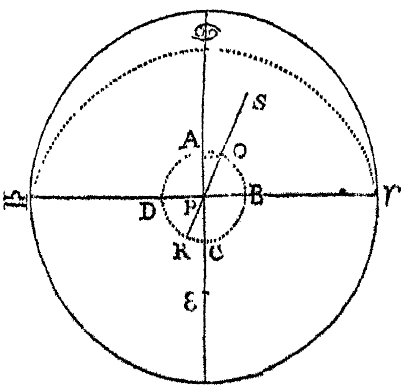
During the Course of my Observations, our ingenious Secretary of the *Royal Society*, Mr. *John Machin*, being employed in considering the Theory of Gravity; and its Consequences, with regard to the Celestial Motions; I acquainted him with the *Phænomena* that I had observed: and at the same time mentioned, *what* I suspected to be the Cause of them. He soon after sent me a Table, containing the Quantity of the annual Precession in the various Positions of the Moon's Nodes, as also the corresponding Nutations of the Earth's Axis; which was computed upon the *Supposition*, that the *mean* annual Precession is 50″, and that the Whole is governed by the Pole of the Moon's Orbit only: and therefore he imagined, that the Numbers in the Table would be too *large*; as in Fact they were found to be. But it appeared, that the Changes which I had observed, both in the annual Precession and Nutation, kept the same Law, as to increasing and decreasing, with the Numbers of his Table. Those were calculated upon the Supposition,

that

that the Pole of the Equator, during a Period of the Moon's Nodes, moved round in the Periphery of a little Circle, whoſe Center was 23° 29′ diſtant from the Pole of the Ecliptic; having itſelf alſo an angular Motion of 50″ in a Year, about the ſame Pole: The North Pole of the Equator was conceived to be in *that* Part of the ſmall Circle, which is fartheſt from the North Pole of the Ecliptic, at the Time when the Moon's Aſcending Node is in the Beginning of *Aries*: and in the oppoſite Point of it, when the ſame Node is in *Libra*.

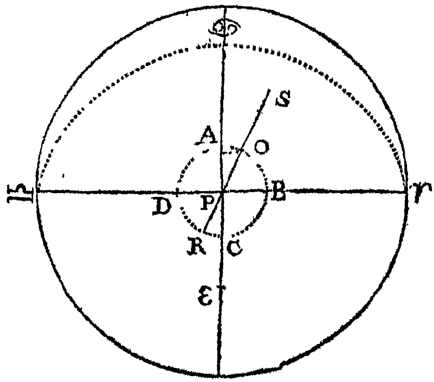
Such a Hypotheſis will account for an Acceleration and Retardation of the annual Preceſſion; as alſo for a Nutation of the Earth's Axis: And if the Diameter of the little Circle be ſuppoſed equal to 18″; which is the whole Quantity of the Nutation, as collected from my Obſervations of γ *Draconis*: then all the *Phænomena* in the ſeveral Stars which I obſerved, will be very nearly ſolved by it.

Let *P* repreſent the mean Place of the Pole of the Equator, about which Point, as a Center, ſuppoſe the true Pole to move in the Circle *ABCD*, whoſe Diameter is 18″. Let *E* be the Pole of the Ecliptic, and *EP* be equal to the mean Diſtance between the Poles of the the Equator and Ecliptic; and ſuppoſe the true Pole



Pole of the Equator to be at *A*, when the Moon's Aſcending Node is in the Beginning of *Aries*; and at *B*, when the Node gets back to *Capricorn*; and at *C*, when the ſame Node is in *Libra*: at which time the North Pole of the Equator being nearer the North Pole of the Ecliptic, by the whole Diameter of the little Circle *AC* equal to 18″; the Obliquity of the Ecliptic will then be ſo much *leſs* than it was, when the Moon's Aſcending Node was in *Aries*. The Point *P* is ſuppoſed to move round *E*, with an equal retrograde Motion, anſwerable to the mean Preceſſion ariſing from the joint Actions of the Sun and Moon: while the true Pole of the Equator moves round *P*, in the Circumference *ABCD*, with a retrograde Motion likewiſe, in a Period of the Moon's Nodes, or of eighteen Years, and ſeven Months. By this means, when the Moon's Aſcending Node is in *Aries*, and the true Pole of the Equator at *A*, is moving from *A* towards *B*: it will approach the Stars, that come to the Meridian with the Sun about the Vernal Equinox; and recede from thoſe that come with the Sun near the Autumnal Equinox, *faſter* than the *mean* Pole *P* does. So that, while the Moon's Node goes back from *Aries* to *Capricorn*, the *apparent* Preceſſion will ſeem ſo much *greater* than the *mean*; as to cauſe the Stars, that lie in the Equinoctial Colure, to have altered their Declination 9″, in about four Years and eight Months, *more* than the mean Preceſſion would do: and in the ſame time, the North Pole of the Equator will ſeem to have approached the Stars, that come to the Meridian with the Sun at our Winter Solſtice, about 9″; and to have receded as *much* from thoſe, that come with the Sun at the Summer Solſtice.

Thus the *Phænomena* before recited are in general conformable to this Hypotheſis. But to be more particular; let *S* be the Place of a Star, *PS* the Circle of Declination paſſing thro' it, repreſenting its Diſtance from the mean Pole, and ♈*PS* its mean Right Aſcenſion. Then if *O* and *R* be the Points, where the Circle of Declination cuts the little Circle *ABCD*; the *true* Pole will be neareſt that Star at *O*, and fartheſt from it at *R*; the whole Difference amounting to 18″, or to the Diameter of the little Circle. As the true Pole of the Equator is ſuppoſed to be at *A*, when the Moon's Aſcending Node is in *Aries*; and at *B*, when that Node gets back to *Capricorn*; and the angular Motion of the true Pole about *P*, is likewiſe ſuppoſed equal to that of the Moon's Node about *E*, or the Pole of the Ecliptic: ſince, in theſe Caſes, the true Pole of the Equator is 90 Degrees before the Moon's Aſcending Node, it muſt be ſo in all others.



When the true Pole is at *A*, it will be at the ſame Diſtance from the Stars that lie in the Equinoctial Colure, as the mean Pole *P* is; for I neglect at preſent the Caſe of ſuch Stars as are *very* near the Pole of the Equator; and as the true Pole recedes back from *A* towards *B*, it will approach the Stars, that lie in that Part of the Colure repreſented by *P*♈; and recede from thoſe, that lie in *P* ♎; not indeed

indeed with an *equable* Motion; but in the *Ratio* of the *Sine* of the Diſtance of the Moon's Node from the Beginning of *Aries*. For if the Node be ſuppoſed to have gone backwards from *Aries* 30°, or to the Beginning of *Piſces*; the Point, which repreſents the Place of the true Pole, will in the mean time, have moved in the little Circle, thro' an Arc, as *AO*, of 30° likewiſe: and would therefore in Effect have approached the Stars that lie in the Equinoctial Colure *P*♈, and have receded from thoſe that lie in *P*♎, $4''\frac{1}{2}$; which is the *Sine* of 30° to the *Radius AP*. For if a Perpendicular fall from *O* upon *P A*, it may be conceived as Part of a great Circle, paſſing thro' the true Pole and any Star lying in the Equinoctial Colure. Now the ſame Proportion, that holds in theſe Stars, will obtain likewiſe in all others; and from hence we may collect a general Rule, for finding how much nearer or farther, any particular Star is, to or from, the *mean* Pole, in any given Poſition of the Moon's Node.

For, if *from the Right-Aſcenſion of the Star, we ſubſtract the Diſtance of the Moon's Aſcending Node from Aries*; *then the Radius will be to the Sine of the Remainder, as* 9'', *is to the Number of Seconds, that the Star is nearer to, or farther from the True, than the Mean Pole*. When that Remainder is *leſs* than 180°, the Star is *nearer* to the True, than to the Mean Pole; and the contrary, when it is *greater* than 180°.

This Motion of the *true* Pole, about the *mean* at *P*, will alſo produce a Change in the Right Aſcenſions of the Stars, and in the Places of the Equinoctial Points; as well as in the Obliquity of the Eclip-

tic: and the Quantity of the Equations, in either of these Cases, may be easily computed for any given Position of the Moon's Nodes. But as it may be needless, to dwell longer on the Explication of the Hypothesis; I shall now proceed to shew its Correspondency with the *Phænomena*, relating to the Alterations of the Polar Distances of some of the Stars which I have observed: by laying before your Lordship the Observations themselves, together with the Computations that are necessary; in order to form a right Judgment about the Cause of these Appearances.

I have endeavoured to find the exact Quantity of the *mean* Precession of the Equinoctial Points, by comparing my own Observations made at *Greenwich*, with those of *Tycho Brahè* and others, which I judged to be most proper for that Purpose. But as many of the Stars, which I compared, gave a different Quantity; I shall assume the mean Result; which gives a Precession of one Degree in seventy-one Years and an half: this agreeing very well likewise with my Observations that were taken at *Wansted*. The Numbers in the following Tables, which express the Change of Declination in each Star, are computed upon the Supposition, that the *mean* Obliquity of the Ecliptic was 23°. 28′. 30″, and that it continued the *same*, during the whole Course of my Observations. And as the Moon's Ascending Node was in the Beginning of *Aries* about the 27th Day of *March* 1727, I have reduced the Place of each Star to *that* Time; by allowing the proper Change of Declination from that Day, to the Day of each respective Observation.

It being also necessary to make an Allowance for the *Aberrations* of Light; I have again examined my

my Obſervations, that were moſt proper to determine the Tranſverſe Axis of the Ellipſis, which each Star ſeems to deſcribe; and have found it to be neareſt to 40″; which Number I therefore make uſe of in the following Computations.

The Diviſions or Points upon the Limb of my Sector are placed five Minutes of a Degree from each other; and are numbered ſo, as to ſhew the Polar Diſtances nearly; the *true* Polar Diſtance exceeding that, which is ſhewn by the Inſtrument, about 1′. 35″. When I firſt began to obſerve, I generally made uſe of *that* Point on the Limb, which was neareſt to the Star's Polar Diſtance, without regarding whether it was more Northerly, or more Southerly than the Star: but as it ſometimes happened, that the Original Point, with which I at firſt compared the Star, became, in Proceſs of Time, pretty remote from it; I afterwards brought the Plummet to another Point, that was nearer to it; and carefully examined, what Number of Revolutions of the Screw of the Micrometer *&c.* correſponded to the Diſtance between the different Points, that I had made uſe of: by which means I was able to reduce all the Obſervations of the ſame Star to the ſame Point, without ſuppoſing the ſeveral Diviſions to be accurately 5′ aſunder.

I have expreſſed the Diſtance of each Star from the Point of the Arc, with which it was compared, in *Seconds* of a Degree and *tenth Parts* of a Second, exactly as it was collected from the Obſervations; altho' I am ſenſible, that the Obſervations themſelves are liable to an Error of more than a *whole* Second; becauſe I meet with ſome, that have been made within two or three Days of each other, that differ 2″, even when they are not marked as *defective* in any reſpect.

It

It would be too tedious, to ſet down the whole Number of the Obſervations that I have made; and therefore I ſhall give only enough of them, to ſhew their Correſpondency with the 'forementioned Hypotheſis in the ſeveral Years, wherein any were made of the Stars here recited. When *ſeveral* Obſervations have been taken of the ſame Star, within a few Days of each other; I have either ſet down the mean Reſult, or *that* Obſervation which beſt agreed with it. I have likewiſe commonly choſen thoſe, that were made near the ſame Seaſon of the Year, in ſuch Stars as gave me the Opportunity of making that Choice; particularly in γ *Draconis*, which was generally obſerved about the End of *Auguſt* or the Beginning of *September*; *That* being the uſual Time, when I went to *Wanſted* on purpoſe to obſerve both *that*, and alſo ſome of the Stars in the *great Bear*. But the Weather proving cloudy at that Seaſon in the Year 1744, prevented my making a ſingle Obſervation, either of γ *Draconis*, or any other Star, while I was there; which is the Cauſe of one Vacancy in a Series of 20 ſucceeding Years, wherein that particular Star had been obſerved. Such Stars, as were either not viſible in the Day-time, towards the Beginning of *September*, or came at ſuch Hours of the Night, as would have incommoded the Family of the Houſe wherein the Inſtrument is fixed, were but ſeldom obſerved, after I went to reſide at *Oxford:* which is the Reaſon, why the Series of Obſervations of *thoſe* is ſo imperfect, as ſometimes to leave a Chaſm for ſeveral Years together. But notwithſtanding this, I doubt not, but upon the whole they will be found ſufficient, to

ſatisfy

satisfie your Lordship of the general Correspondency between the *Hypothesis* and the *Phænomena*, in the several Stars; however different their Situations are, with respect to the Cardinal Points of the Equator.

As I made more Observations of γ *Draconis* than of any other Star; and it being likewise very near the Zenith of *Wansted*; I will begin with the Recital of some of them. The Point upon the Limb, with which this Star was compared, was 38°. 25′ from the North Pole of the Equator, according to the Numbers of the Arc of my *Sector*. The first Column, in the following Table, shews the Year and the Day of the Month, when the Observations were made; the next gives the Number of *Seconds*, that the Star was found to be *South* of 38°. 25′: the third contains the Alterations of the Polar Distance, which the *mean* Precession, at the rate of one Degree in 71½ Years, would cause in this Star, from the 27th Day of *March* 1727, to the Day on which the Observation was taken: the fourth shews the Aberrations of Light: the fifth, the Equations arising from the 'forementioned Hypothesis: and the sixth gives the *mean* Distance of the Star from the Point with which it was compared, found, by collecting the several Numbers, according to their Signs, in the 3d, 4th, and 5th Columns, and applying them to the *Observed Distances* contain'd in the Second.

If the Observations had been perfectly exact, and the several Equations of their *due* Quantity; then all the Numbers in the last Column would have been equal; but since they differ a little from one another; if the *mean* of All be taken, and and the Extremes are

pared with it, we ſhall find no greater Difference, than what may be ſuppoſed to ariſe from the Uncertainty of the Obſervations themſelves; it no where amounting to more than $1''\frac{1}{2}$. The Hypotheſis therefore ſeems, in this Star, to agree extremely well with the Obſervations here ſet down; but as I had made above 300 of it; I took the Trouble of comparing each of them with the Hypotheſis: and altho' it might have been expected, that, in ſo large a Number, ſome great Errors would have occurred; yet there are very few, *viz.* only eleven, that differ from the mean of theſe ſo much as $2''$; and not one that differs ſo much as $3''$. This ſurpriſing Agreement, therefore, in ſo long a Series of Obſervations, taken in all the various Seaſons of the Year, as well as in the different Poſitions of the Moon's Nodes, ſeems to be a ſufficient Proof of the Truth, both of *this* Hypotheſis, and alſo of *that* which I formerly advanced, relating to the Aberrations of Light; ſince the Polar Diſtance in this Star may differ, in certain Circumſtances, almoſt a Minute, *viz.* $56''\frac{1}{2}$, if the Corrections reſulting from both theſe Hypotheſes are neglected; whereas, when thoſe Equations are rightly applied, the mean Place of the Star comes out the ſame, as nearly, as can be reaſonably expected.

γ Draconis			South of ° ′ 38. 25	Precesſion.	Aberration.	Nutation	Mean Diſt
			″	″	″	″	″
1727	September	3	70 5	— 0 4	+ 19 2	— 8 9	80.4
1728	March	18	108.7	— 0.8	— 19 0	— 8.6	80.3
	September	6	70.2	— 1.2	+ 19 3	— 8 1	80.2
1729	March	6	108.3	— 1 6	— 19 3	— 7.4	80.0
	September	8	69 4	— 2.1	+ 19 3	— 6.9	80.2
1730	September	8	68.0	— 2 9	+ 19.3	— 3 4	80.5
1731	September	8	66.0	— 3.8	+ 19.3	— 1 0	80.5
1732	September	6	64.3	— 4 6	+ 19.3	+ 2 0	81.0
1733	Auguſt	29	60.8	— 5.4	+ 19.0	+ 4 8	79.2
1734	Auguſt	11	62 3	— 6.2	+ 16.9	+ 6.9	79.9
1735	September	10	60.0	— 7 1	+ 19.3	+ 7.9	80 1
1736	September	9	59.3	— 8 0	+ 19.3	+ 9.0	79.6
1737	September	6	60.8	— 8.8	+ 19.3	+ 8 5	79.8
1738	September	13	62.0	— 9 6	+ 19 3	+ 7 0	78.7
1739	September	2	66.6	— 10 5	+ 19 2	+ 4.7	80.0
1740	September	5	70 8	— 11.3	+ 19.3	+ 1.9	80.7
1741	September	2	75 4	— 12.1	+ 19.2	— 1 1	81 4
1742	September	5	76.7	— 12 9	+ 19.3	— 4.0	79.1
1743	September	2	81.6	— 13.7	+ 19 1	— 6.4	80 6
1745	September	3	86.3	— 15.4	+ 19.2	— 8.9	81.2
1746	September	17	86.5	— 16.2	+ 19 2	— 8.7	80.8
1747	September	2	86.1	— 17.0	+ 19 2	— 7.6	80.7

I made about 250 Obſervations of *β Draconis*; which I find correſpond as well with the Hypotheſis, as thoſe of *γ*; but ſince the Poſitions of both theſe Stars, in reſpect to the Solſtitial Colure, differ but little from each other; it will be needleſs to ſet down the Obſervations of *β*. I ſhall therefore proceed to lay before your Lordſhip, ſome Obſervations of a ſmall Star, that is almoſt oppoſite to *γ Draco-*

conis in Right Aſcenſion, being the 35th *Camelopardali Hevel.* in the *Britiſh* Catalogue. Mr. *Flamſteed*, indeed, has not given the Right Aſcenſion of this Star; but *that* being neceſſary to be known, in order to compute the Change of its Declination ariſing from the Preceſſion of the Equinox; I compared the Time of its Tranſit over the Meridian, with that of ſome other Stars near the ſame Parallel; whereby I found, that its Right Aſcenſion was 85°. 54.′½ at the Beginning of the Year 1737.

This ſmall Star was compared with the ſame Point of the Limb of my Sector, as γ *Draconis*; and the ſecond Column, in the following Table, ſhews how many *Seconds* it was found to be South of that Point, at the time of each reſpective Obſervation. The other Columns contain, as in the foregoing Table, the Equations that are neceſſary to find, what its *mean* Diſtance from the ſame Point would have been on the 27th Day of *March* 1727, which is exhibited in the laſt Column. The whole Number of my Obſervations of this Star did not much exceed forty; the greateſt Part of which were made before the Year 1730; in ſome of the following Years none were taken; and only a ſingle one in any other, except in 1739. However, their Correſpondency ſeems ſufficient to evince the Truth of the Hypotheſis: for if the Mean of theſe, contain'd in the Table, be taken, not one, among the reſt of the Obſervations, will differ from it more than 2″.

35th *Camelopard. Hevelii.*		South of ° ′ 38 25	Precession.	Aberration.	Nutation.	Mean Dist South.
		″	″	″	″	″
1727 October	20	73.6	+ 0.9	− 6 7	+ 8 9	76.7
1728 January	12	60.8	1.2	+ 6.1	8.8	76.9
March	1	57.8	1.4	+ 9.4	8.7	77.3
September	26	75.2	2.3	− 8.8	8 1	76 8
1729 February	26	56.4	2.8	+ 9.4	7.6	76 2
1730 March	3	57.8	4 4	9 4	5.4	77.0
1731 February	5	59 1	5 6	8 5	+ 3 0	76 2
1733 January	31	64 1	8 7	8 2	− 2 9	78.1
1738 December	30	61.8	17.2	4 3	6 5	76.8
1739 February	4	56.9	17.3	8 5	6 3	76 4
1740 January	20	56.0	18.6	7.0	− 4.0	77.6
1747 February	27	32.3	28.5	9.4	+ 8.4	78 6

The Obſervations of the foregoing Stars are the moſt proper, to prove the Change of the Inclination of the Earth's Axis to the Plane of the Ecliptic; thoſe, which follow, will ſhew in what manner the Stars, that lie near the Equinoctial Colure, are affected, as well as others, that are differently ſituated, with reſpect to the Cardinal Points of the Equator. Some of theſe Stars are indeed more remote from the Zenith, than I would have choſen, if there had been others, of equal Luſtre, in more proper Poſitions; becauſe Experience has long ſince taught me, that the Obſervations of ſuch Stars, as lie near the Zenith, do generally agree beſt with one another, and are therefore the fitteſt to prove the Truth of any Hypotheſis. I ſhall begin with thoſe near the Vernal Equinox. α *Caſſiopeæ* was compared with the Point marked 34°. 55′; and at firſt was

was found to be more *Southerly*, but afterwards became more *Northerly* than that Point, as in the following Table; the laſt Column of which ſhews its mean Diſtance *South* of that Point on the 27th of *March* 1727. The Obſervation of the 23d Day of *December*, in the Year 1738, differs 3″ from the *mean* of the others; as does alſo another, that was taken five Days after this; neither of which being marked as uncertain, I judged it proper to inſert one of them; altho' they give the mean Place of the Star near 2 Seconds more *Northerly* than any other, in a Series of above 100; *all* of which correſpond, with the *mean* of theſe here recited, within leſs than 2″; excepting *two*, that give the Stars mean Diſtance almoſt 3″ more *Southerly*; but theſe laſt mentioned are marked as dubious; and indeed they appear to have been bad, by comparing them with ſeveral others, that were made near the ſame time, from which they differ almoſt 2″.

α *Caſſiopeæ*.		South of 34°. 55′	Preceſſion.	Aberration.	Nutation.	Mean Diſt. South
		″	″	″	″	″
1727 September	9	55.0	+ 9.0	+ 2.2	+ 2.4	68.6
1728 September	17	30.8	29.4	+ 4.6	5.2	70.0
1729 June	8	35.7	43.8	− 16.3	6.8	70.0
December	3	N. 9.4	53.5	+ 16.5	7.7	68.3
1730 June	11	S. 13.8	64.0	− 16.2	8.4	70.0
December	9	N. 30.8	73.8	+ 16.3	8.8	68.1
1732 January	8	N. 49.2	95.4	12.9	8.9	68.0
1733 January	21	64.8	116.0	+ 10.0	7.9	69.1
1734 June	13	62.8	143.8	− 16.1	5.0	69.9
December	11	105.4	153.7	+ 16.2	+ 3.7	68.2
1738 December	23	176.3	234.0	+ 15.2	− 7.2	65.7
1740 June	2	169.1	262.8	− 16.5	− 8.9	68.3
1747 February	27	332.3	397.0	+ 0.2	+ 4.7	69.6

Altho'

Altho' I have taken no Obſervation of *τ Perſei* ſince the 22d Day of *January* 1740; yet, as this Star is very near the Zenith, and a ſufficient Number were made about the Times when the Equation, reſulting from the Hypotheſis, was at its *Maximum*; I judged it proper to inſert ſome of them in the next Table; the laſt Column of which ſhews, how much the Star's *mean* Diſtance was *South* of 38°. 20′. on the 27th Day of *March* 1727. Among near 60 Obſervations I meet with two only, that differ from the mean of theſe ſo much as 2″; and thoſe differ almoſt as much from the mean of others, that were taken near the ſame time: ſo that the Hypotheſis ſeems to correſpond, in general, with the Obſervations of this Star as well, as with either of the foregoing.

τ Perſei.	South of 38°. 20′	Preceſſion.	Aberration.	Nutation.	Mean Diſt. South.
	″	″	″	″	″
1727 September 16	60.1	+ 7.4	− 3.2	+ 6.7	71.0
December 29	39.7	11.9	+ 12.9	7.2	71.7
1728 December 21	22.5	27.2	12.8	8.7	71.2
1729 December 2	S. 9.2	42.0	11.5	9.0	71.7
1731 January 3	N. 8.2	59.0	12.8	8.3	71.9
1732 January 8	22.0	74.8	12.7	6.7	72.2
1733 January 21	34.6	91.0	11.7	+ 4.3	72.4
1738 December 23	117.0	183.4	12.8	− 9.0	70.2
1740 January 22	132.5	200.2	11.7	8.6	70.8

After the laſt recited Obſervations, it may perhaps ſeem needleſs to add thoſe of *α Perſei*, which is farther from the Zenith; but however, as this Star lies very nearly at an equal Diſtance from the Equinoctial

Equinoctial and Solstitial Colures, and the Series of Observations of it is somewhat more complete, than that of τ *Persei*; I shall insert one at least, for each Year wherein it has been observed; whereby it may appear, that the Hypothesis solves the *Phænomena* of Stars in this Situation, as exactly as in others: for if a *mean* be taken of the Numbers in the last Column of the following Table, which expresses the *mean* Distance of the Star *South* of 41°. 5′. on *March* 27th 1727, it will agree within two Seconds, with every one of 80 Observations, that have been made of this Star.

α *Persei*	South of ° ′ 41. 5	Precession.	Aberration.	Nutation	Mean Dist South
	″	″	″	″	″
1727 December 29	79.4	+ 10.5	+ 11.4	+ 7.9	109.2
1728 April 7	87.5	14.3	− 0.8	8.2	109.2
July 5	94.6	17.7	− 11.4	8.5	109.4
December 13	65.7	23.8	+ 10.6	8.8	108.9
1729 December 3	53.4	37.2	9.7	8.9	109.2
1731 January 3	38.6	52.3	11.4	7.8	110.1
1732 January 8	26.8	66.2	+ 11.4	+ 5.9	110.3
1734 July 11	S. 21.3	101.0	− 11.4	− 1.1	109.8
1738 December 24	N. 56.3	162.6	+ 11.2	9.0	108.5
1740 January 21	71.8	177.4	10.9	− 8.2	108.3
1747 February 27	182.5	275.4	6.6	+ 8.5	108.0

Having already given Examples of Stars, lying near both the Solstices and the *Vernal* Equinox; I shall now add the Observations of *one*, that is not far from the *Autumnal* Equinox, *viz.* η *Ursæ Majoris*, the brightest Star in that Part of the Heavens, which approaches the Zenith of *Wansted* within a Degree;

and

and which, by reaſon of its Luſtre and Poſition, gave me the Opportunity of making my Series of Obſervations of *It*, more complete than of many others. This Star was compared with the Point marked 39°. 15′. and was *South* of it as in the following Table; wherein your Lordſhip will ſee, that the Obſervations of the Years 1740 and 1741 give the Polar Diſtances 3″ greater, than the *mean* of the other Years. Had there been only a ſingle Obſervation taken in either of thoſe Years, Part of this apparent Difference might have been ſuppoſed to ariſe from their Uncertainty; but as there were 8 Obſervations taken within a Week, either before or after the 3d Day of *June* 1740, which agree well with each other; and three were made within 20 Days in *September* 1741, which likewiſe correſponded with each other; I am inclined to think, that the 'forementſon'd Differences muſt be owing to ſomething elſe, beſides the Error of the Obſervations. This *Phænomenon* therefore may deſerve the Conſideration of thoſe Gentlemen, who have employed their Time in making Computations relating to the Quantity of the Effects, which the Power of Gravity may, on various Occaſions, produce. For I ſuſpect, that the Poſition of the Moon's Apogee, as well as of her Nodes, has ſome Relation to the apparent Motions of the Stars that I am now ſpeaking of.

My Series of Obſervations of ſeveral Stars abound, of late Years, with ſo many and long Interruptions; that I cannot pretend to *determine* this Point; but probably the Differences before taken notice of in the Obſervations of *α Caſſiopeæ*, and ſome others

E that

that I have found likewise among the Observations of *other* Stars, that are not here recited, may be owing to such a Cause; which, altho' it should not have any large Share of Influence, may yet, in certain Circumstances, discover a Defect in a Hypothesis, that pays no Regard at all to *It*. But whether these Differences do arise from the Cause already hinted at; or whether they proceed from any Defect of the Hypothesis itself in any other respect; it will not be very *material* in point of Practice; since *that* Hypothesis, as it was before laid down, appears to be sufficient to solve all the *Phænomena*, to as great a Degree of Exactness, as we can in general *hope* or *expect* to make Observations. For if I take the *mean* of all the Numbers in the last Column of the following Table for η *Ursæ Majoris*, and compare it with any one of 164 Observations that were taken of it, the Difference will not exceed three Seconds.

η *Ursæ Majoris*			South of 39° 15'	Precession.	Aberration.	Nutation	Mean Dist. South.
			"	"	"	"	"
1727	October	17	153.3	− 10.2	+ 1.0	− 5.2	138.9
1728	January	24	176.4	15.2	− 17.6	5.8	137.8
	July	17	150.8	23.9	+ 17.8	6.9	137.8
	October	11	170.6	28.2	+ 2.6	7.3	137.7
1729	January	16	196.6	33.1	− 17.8	7.8	137.9
	July	21	170.4	42.4	+ 17.8	8.4	137.4
1730	July	19	189.6	60.6	+ 17.8	9.0	137.8
	December	28	232.4	68.7	− 16.7	8.9	138.1
1731	September	18	218.1	81.9	+ 9.4	8.4	137.2
1732	January	10	250.7	87.7	− 17.7	8.0	137.3
	April	13	238.7	92.3	− 0.8	7.7	137.9
1734	J ly	11	255.7	133.3	+ 17.6	− 2.3	137.7

η *Ursæ*

η *Ursæ Majoris*	South of ° ′ 39 15	Preceſſion.	Aberration.	Nutation	Mean Diſt South
	″	″	″	″	″
1735 September 10	280.8	154.6	+ 11.4	+ 1.2	138.8
1736 September 8	294.7	172.8	11.6	4.1	137.6
1737 July 3	303.0	187.8	17.2	6.1	138.5
1738 June 29	319.0	205.8	16.8	7.9	137.9
1739 April 25	348.0	220.8	2.5	8.8	138.5
1740 June 3	360.3	241.1	12.8	8.9	140.9
1741 September 23	390.9	265.0	7.9	+ 7.4	141.2
1745 September 5	466.7	337.1	12.4	− 3.3	138.7
1746 September 20	492.0	356.2	8.8	5.9	138.7
1747 September 2	507.2	373.5	13.2	7.8	139.1

You may perceive, my Lord, by inſpecting the Tables which contain the Obſervations of α *Caſſiopeæ* and η *Urſæ Majoris*; that the greateſt Differences that occur therein may be diminiſhed, by ſuppoſing the *true* Pole of the Equator to move round the Point *P*, in an *Ellipſis*, inſtead of a Circle. For if the tranſverſe Axis, lying in the Direction *AC*, be 18″, and the Conjugate, as *DB*, be about 16″; the Equations, reſulting from ſuch an Hypotheſis, will make the Numbers in the laſt Columns agree with each other, nearer than as they now ſtand. But ſince this would not entirely remove the Inequalities, in all the Poſitions of the Moon's Nodes; I ſhall refer the more accurate Determination of the *Locus* of the *true* Pole to Theory; and at preſent only give the Equations for the Preceſſion of the Equinoctial Points, and the Obliquity of the Ecliptic, as alſo the real Quantity of the annual Preceſſion, to every 5th Degree of the Place of the Moon's Aſcending Node, in the following Tables; juſt as

 they

they reſult from the Hypotheſis, as at firſt laid down; it appearing, from what has already been remark'd, that theſe will be ſufficiently exact for Practice in all Caſes.

The Equation of the Equinoct. Points

☽s ☊ from ♈	Sig. 0 / Sig. VI	I / VII	II / VIII	Subſt. / Add
°	″	″	″	°
0	0.0	11.3	19.6	30
5	2.0	13.0	20.5	25
10	3.9	14.5	21.2	20
15	5.8	16.0	21.8	15
20	7.7	17.3	22.2	10
25	9.6	18.5	22.5	5
30	11.3	19.6	22.6	0
Subſt. / Add	Sig. V / Sig. XI	IV / X	III / IX	☽s ☊ from ♈

The Equation of the Obliquity of the Eclıptick.

☽s ☊ from ♈	Sig. 0 / Sig. VI	I / VII	II / VIII	Add / Subſt.
°	″	″	″	°
0	9.0	7.8	4.5	30
5	9.0	7.4	3.8	25
10	8.9	6.9	3.1	20
15	8.7	6.4	2.3	15
20	8.5	5.8	1.6	10
25	8.2	5.2	0.8	5
30	7.8	4.5	0.0	0
Add / Subſt.	Sig. V / Sig. XI	IV / X	III / IX	☽s ☊ from ♈

The Annual Preceſſion of the Equinoctial Points.

☽ ☊ from ♈	Sig. 0	I	II	III	IV	V	
°	″	″	″	″	″	″	°
0	58.0	57.0	54.2	50.3	46.5	43.7	30
5	57.9	56.6	53.6	49.7	46.0	43.4	25
10	57.9	56.2	53.0	49.0	45.5	43.2	20
15	57.7	55.7	52.3	48.4	45.0	43.0	15
20	57.5	55.2	51.7	47.7	44.5	42.8	10
25	57.3	54.7	51.0	47.1	44.1	42.8	5
30	57.0	54.2	50.3	46.5	43.7	42.7	0
	Sig. XI	X	IX	VIII	VII	VI	☽s ☊ from ♈

Sir

Sir *Iſaac Newton*, in determining the Quantity of the annual Preceſſion from the Theory of Gravity, upon Suppoſition that the Equatorial is to the Polar Diameter of the Earth as 230 is to 229, finds the Sun's Action ſufficient to produce a Preceſſion of $9''\frac{1}{8}$ only; and, collecting from the Tides the Proportion between the Sun's Force and the Moon's to be as 1 to $4\frac{1}{2}$, he ſettles the mean Preceſſion, reſulting from their joint Actions, at $50''$. But ſince the Difference between the Polar and Equatorial Diameter is found, by the late Obſervations of the Gentlemen of the *Academy of Sciences*, to be greater than what Sir *Iſaac* had computed it to be; the Preceſſion, ariſing from the Sun's Action, muſt likewiſe be greater than what he has ſtated it at, nearly in the ſame Proportion. From whence it will follow, that the Moon's Force muſt bear a leſs Proportion to the Sun's than $4\frac{1}{2}$ to 1; and perhaps the *Phænomena*, which I have now been giving an Account of, will ſupply the beſt *Data* for ſettling this Matter.

As I apprehend, that the Obſervations already ſet down will be judged ſufficient, to prove in general the Truth of the Hypotheſis before advanced; I ſhall not trouble your Lordſhip with the Recital of more, that I made of Stars lying at greater Diſtances from the Zenith; thoſe not being ſo proper, for the Reaſon before-mention'd, to eſtabliſh the Point that I had chiefly in View. But as it may perhaps be of ſome Uſe to future Aſtronomers, to know what were the *mean* Differences of Declination, at a given Time, between ſome Stars, that lie nearly oppoſite to one another in Right Aſcenſion, and not far from either of the *Colures*; I ſhall ſet down the Reſult of the Compariſon of a few, that differ ſo little in Declination,

that

that I could determine the Quantity of that Difference with great Certainty.

By the *mean* of 64 Obſervations, that were made of *α Caſſiopeæ* before the End of the Year 1728, I collect, after allowing for the Preceſſion, Aberration and Nutation as in the foregoing Tables; that the *mean* Diſtance of this Star was 68″.7 South of 34°. 55′, on the 27th Day of *March* 1727. By a like Compariſon of 40 Obſervations, taken of γ *Urſæ Majoris* during the ſame Interval of Time, I find this Star was, at the ſame time, 39″.6 South of 34°. 45′. I carefully meaſured, with the Screw of the Micrometer, the Diſtance between the Points, with which theſe Stars were compared; and found them to be 9′. 59″ from each other, or one Second leſs than they ought to have been. Hence it follows, that the *mean* Difference of Declination between theſe two Stars, was 10′. 28″.1, on the 27th Day of *March* 1727.

By the *mean* of 65 Obſervations, that were taken of *β Caſſiopeæ*, before the End of the Year 1728, this Star was 25″.8 North of 32°. 20′, on the 27th Day of *March* 1727: and by the *mean* of 52 Obſervations, ε *Urſæ Majoris* was 87″.6 South of 32°. 30′ at the ſame time. The Diſtance between theſe Points was found to be 9′. 59″.3; from whence it follows, that the *mean* Difference of Declination between theſe two Stars was 11′. 52″.7 on *March* 27th 1727.

By the *mean* of 100 Obſervations, taken before the End of the Year 1728, the *mean* Diſtance of γ *Draconis* was 79″.8 South of 38°. 25′ on *March* 27th 1727; and by the *mean* of 35 Obſervations, the

the 35th *Camelopard. Hevel.* was South of the ſame Spot 76″.4. So that the mean Polar Diſtance of γ *Draconis* was only 3″.4 greater, than that of the 35th *Camelopard. Hevel.* but as the Equation for the Nutation, in both theſe Stars, was then near the *Maximum*, and to be applied with contrary Signs; the *Apparent* Polar Diſtance of γ *Draconis* was 21″.4 greater, on the 27th Day of *March* 1727.

The Differences of the Polar Diſtances of the Stars, as here ſet down, may be preſumed, both on account of the Radius of the Inſtrument and the Number of Obſervations, to be very exactly determined, to the Time when the Moon's Aſcending Node was at the Beginning of *Aries*; and if a like Compariſon be hereafter made, of Obſervations taken of the ſame Stars, near the ſame Poſition of the Moon's Nodes; future Aſtronomers may be enabled, to ſettle the Quantity of the mean Preceſſion of the Equinox, ſo far as it affects the Declination of theſe Stars, with great Certainty: and they may likewiſe diſcover, by means of the Stars near the Solſtitial Colure, from what Cauſe the apparent Change in the Obliquity of the Ecliptic really proceeds, if the mean Obliquity be found to diminiſh gradually.

The 'forementioned Points indeed can be ſettled only on the Suppoſition, that the angular Diſtances of theſe Stars do continue always the ſame, or that they have no real Motion in themſelves; but are at Reſt in Abſolute Space. A Suppoſition, which though uſually made by Aſtronomers, nevertheleſs ſeems to be founded on too uncertain Principles, to be admitted in all Caſes. For if a Judgment may be formed, with Regard to this Matter, from the Reſult

ſult of the Compariſon of our beſt modern Obſervations, with ſuch as were formerly made with any tolerable Degree of Exactneſs; there appears to have been a real Change in the Poſition of ſome of the fixed Stars, with reſpect to each other; and ſuch, as ſeems independent of any Motion in our own Syſtem, and can only be referred to ſome Motion in the Stars themſelves. *Arcturus* affords a ſtrong Proof of this: for if its preſent Declination be compared with its Place, as determined either by *Tycho* or *Flamſteed*, the Difference will be found to be much greater, than what can be ſuſpected to ariſe from the Uncertainty of their Obſervations.

It is reaſonable to expect, that other Inſtances of the like kind muſt alſo occur among the great Number of the viſible Stars: becauſe their relative Poſitions may be alter'd by various means. For if our own Solar Syſtem be conceived to change its Place, with reſpect to Abſolute Space; this might, in Proceſs of Time, occaſion an apparent Change in the angular Diſtances of the fixed Stars; and in ſuch a Caſe, the Places of the neareſt Stars being more affected, than of thoſe that are very remote; their relative Poſitions might ſeem to alter; tho' the Stars themſelves were really immoveable. And on the other Hand, if our own Syſtem be at Reſt, and any of the Stars really in Motion, this might likewiſe vary their apparent Poſitions; and the more ſo, the nearer they are to us, or the ſwifter their Motions are, or the more proper the Direction of the Motion is, to be rendered perceptible by us. Since then the Relative Places of the Stars may be changed from ſuch a Variety of Cauſes, conſidering that amazing Di-

tance at which it is certain ſome of them are placed, it may require the Obſervations of many Ages, to determine the Laws of the apparent Changes, even of a ſingle Star: much more difficult therefore muſt it be, to ſettle the Laws relating to all the moſt remarkable Stars.

When the Cauſes, which affect the Places of all the Stars in general are known; ſuch as the Preceſſion, Aberration, and Nutation; it may be of ſingular Uſe, to examine nicely the relative Situations of particular Stars: and eſpecially of thoſe of the greateſt Luſtre, which, it may be preſumed lie neareſt to us, and may therefore be ſubject to more ſenſible Changes; either from their own Motions, or from that of our Syſtem. And if at the ſame time that the brighteſt Stars are compared with each other, we likewiſe determine the relative Poſitions of ſome of the *ſmalleſt* that appear near them, whoſe Places can be aſcertained with ſufficient Exactneſs; we may perhaps be able to judge to what Cauſe the Change, if any be obſervable, is owing. The Uncertainty that we are at preſent under, with reſpect to the Degree of Accuracy wherewith former Aſtronomers could obſerve, makes us unable to determine ſeveral Things, relating to the Subject that I am now ſpeaking of: but the Improvements, which have of late Years been made in the Methods of taking the Places of the heavenly Bodies, are ſo great, that a few Years may hereafter be ſufficient, to ſettle ſome Points; which cannot now be ſettled, by comparing even the earlieſt Obſervations with thoſe of the [illegible]

[illegible]

It were to be wish'd therefore, that such Persons as are provided with proper Instruments, would attempt to determine, with great Care, the present relative Positions of several of the Principal Stars, in various Parts of the Heavens; especially of those, that are least affected by Refraction: *that* Cause having many times so uncertain an Influence on the Places of Objects, that are very remote from the Zenith; that wherever *It* is concerned, the Conclusions, deduced from Observations that are *much* affected by it, will always remain doubtful, and too precarious, in many Cases, to be relied upon.

The Advantages, arising from different Persons attempting to settle the same Points of Astronomy near the same time, are so much the greater; as a Concurrence in the Result, would remove all Suspicion of Incorrectness in the Instruments made use of. For which Reason, I esteem the curious *Apparatus* at *Shirburn Castle*, and the Observations there taken, as a most valuable *Criterion*, whereby I may judge of the Accuracy of those, that are made at the *Royal Observatory*; and as a Lover of Science I cannot but wish, that our Nation abounded with more frequent Examples, of Persons of like Rank and Ability with your Lordship, equally desirous of promoting *This*, as well as every other Branch of Natural Knowledge, that tends to the Honour and Benefit of our Country.

But were the Patrons of Arts and Sciences ever so numerous, the Subject of my present Letter is of such a Nature as must direct me to beg Leave to address it to the *Earl of Macclesfield*; not only as a most competent Judge of it, but as the *sole* Person,

ſon, in this Nation, that hath Inſtruments proper to examine into the Truth of the Facts here related. And it is a particular Satisfaction to me, that after ſo long an Attendance upon theſe *Phænomena*, I am allowed the Honour of tranſmitting the Account of them to the Public, thro' your Lordſhip's Hands: as it gives me at the ſame time an Opportunity of profeſſing the grateful Senſe I ſhall ever retain, both of the ſignal Favours which I formerly received from the noble Earl your Father, and of the many recent Obligations conferr'd by yourſelf upon,

My LORD,

Your Lordſhip's

moſt obedient

humble Servant,

Greenwich, Dec. 31. 1747.

Ja. Bradley.

II. *A Letter to* Martin Folkes *Esq. LL. D. Pr. R. S. containing some Observations upon certain* Shell-Fish *(lodg'd in a large Stone brought from* Mahon *Harbour by Mr.* Samuel More, *Purser of the* Sterling-Castle *Man of War) from* James Parsons *M. D. F. R. S.*

Read Jan. 21. 1747. THE Favour Mr. *More* and Mr. *Graham* have done the *Royal Society*, by their Present of the Stone containing the shell'd Fish, and called the *Dottle-Stone*, has given Occasion to the following Hints, to shew of what sort these Fish are, and that they are rare and curious to us.

Upon our first hearing of them by Mr. *More*'s Letter to *Richard Graham* Esq; because it was said the Fish were lodged in a Stone, it was thought they were the same we know by the Name of *Pholades*, and of which there are Plenty upon our own Coasts; but I believe none of us had seen these before, nor had a Notion of any others being lodged in Stone but our *Pholades* above mention'd; whereas these seem to be peculiar to the Mediterranean Sea, since *Rondeletius*, and after him *Aldrovandus*, have given their Account and Figures of them, among others that are Inhabitants of the same Regions.

These two Authors call them simply *Pholades*, which Term is derived of the *Greek* Verb φωλέω, and signifies, to have a hiding Place; every Animal therefore

therefore that abſconds in Earth or Stones might be called ſo too: Hence I think, that φωλὰς is too vague and inſignificant a Name for any particular Animal, and that ſome other, which has a nearer Relation to its generical Character ought to be given to it.

If *Aldrovandus* had ſeen thoſe, which we as erroneouſly call *Pholades*, incloſed in their Cells, he would, no doubt, have call'd them by the ſame Name, and for the ſame Reaſon; but I am inclined to think he never ſaw the intire Fiſh, but only the Shell; becauſe he gives a very imperfect Deſcription of it, among ſeveral others which he has Figures of alſo, and which he calls *Conchæ longæ Authoris aliæ*, which follow an Account and Figure of the *Concha longa* of *Pliny*.

Dr. *Lister* and *Rumphius* alſo have Figures of this Mediterranean Fiſh, and, after that Author, call it *Pholas*: But ſince this Term barely denotes the Place of its Reſidence, let us endeavour to give it a proper Name, which may be done by conſidering its Similarity with ſome *Genus* already known.

The above-mention'd Gentleman ſays, the Stones are from half a hundred to four or five hundred weight each, lying at all Depths to twenty Feet under Water; full of Cells, each containing a ſingle Fiſh, call'd by the Inhabitants the *Dottle* Fiſh; which Name he judiciouſly ſuppoſes to be a Corruption of the Word *Dactylus* from their Form. He alſo ſays, the Fiſh is of the ſame Nature with the common Muſcle, but much more delicious, and their eating them is never attended with thoſe poiſonous Symptoms, that have been often thought to be caus'd by eating Muſcles.

The

The Shells are indeed, in all Respects but one, like the *Mytulus vulgaris*, or *common Muscle*; these being small at the Hinge-End, and having a broad thin Edge at the Opposite; whereas the former are nearly equal at both Ends, as well as strait and somewhat depressed; and as to the Structure of the Fish of both, they are alike, tho' with this small Difference, that the *Lingula* of the common Muscle is detached towards the Point, and that of the other is confin'd all along. I therefore submit it, whether either of the following Names would not properly express it,

Mytulus cylindroides, the cylindroid Muscle, or *Mytulus Dactyliformis*, the *Date Muscle*. Its external Form conducing much to encourage the latter, which Mr. *More* has hinted at, in calling the Stone the *Dottle*, *Dotting*, or *Dating-Stone*; for as to the Place of its Residence, that belongs rather to its general natural History than its distinguishing Name.

Doctor *Lister*, and after him Monsieur *D'Argensville* have drawn our *Pholas* with five Shells; but we have some Reason to suspect they are only Bivalves; for, upon examining those inclosed in the *Specimen* before you, in Company with Mr. *Hill*, none of them appeared to have any more than two Shells. And in a Specimen (given to Mr. *Peter Collinson* by Sir *Charles Wager*) of one of these Fish, which lodg'd itself in the Bottom of a Ship, there were but two Shells found; which Shell together with the Piece of Wood, wherein it was lodged, I myself saw along with several other Friends in the above-mention'd Gentleman's Collection. Now these ought, in like manner, after its generical

Character,

Character, to be ranged among the *Chamæ*; and as they have a *Proboscis* which none of the *Mytuli* have, I would also offer the following Name for this Fish;

Chama longa rugis asperis, alba. The long rough white *Chama.*

Mr. *Baker* has shewn me another Species of *Pholas*, which he lately took out of a Stone from the Coast of *Cornwal*, and which has more of the *Pectuncle* than any other kind, in its Form, *Cardo*, and shutting close; which the Shells just mention'd cannot do: Now these three distinct Kinds of Shell-Fish can never be said to be rightly called by the single Name of *Pholas.*

The common Objection to these Fish boring their Way into the Stones in which they are found, *viz.* that the Stones are first in a soft State, and so harden about them, may be obviated by the following Considerations: First, that in Mr. *More*'s great Stone, when it was broken, there appear'd thro' its Substance several petrify'd fossil Shells; which clearly shew that its Formation was of an ancienter Date than the Age of these Muscles can admit of. Secondly, That the Holes on the Surface are narrower, in general, than the Cavity in which the Fish lies; and which demonstrates, that they enter young, and are capable of enlarging their Room as they grow bigger, by abrading the Sides of their Cells: And this is further apparent, by the sandy Matter found in the Bottoms of those Cells, which the Fish cannot well get rid of, when it happens, that the Orifice is higher than the Bottom; Abundance of which

Mr.

Mr. *Hill* and myſelf obſerved in ſome of the Holes; and which is eaſily thrown quite out, when the Orifice is depending; for in theſe we obſerved none: And this is further confirmed by what Dr. *Woodward* relates in the firſt Volume of his Catalogue *, that certain Pillars of white *Carrara* Marble taken out of the Sea, on the Coaſt of *Legborn*, after lying there a Number of Years, were deſtroy'd by the boring of theſe *Pholades*.

As to the Manner of their penetrating the Stones, I cannot give the leaſt Account of it; who am,

SIR,

Your moſt obedient Servant,

James Parſons.

* Page 25.

III. *A Collection of the* Electrical Expeririments *communicated to the* Royal Society *by* Wm. Watſon, *F. R. S. read at ſeveral Meetings between* October 29. 1747. *and* Jan. 21. *following.*

I.

An Account of the Experiments made by ſeveral Gentlemen of the Royal Society, *in order to diſcover whether or no the electrical Power, when the Conductors thereof were not ſupported by Electrics* per se *would be ſenſible at great Diſtances: With an Inquiry concerning the reſpective Velocities of* Electricity *and* Sound: *To which is added an Appendix, containing ſome further Inquiries into the Nature and Properties of* Electricity.

Read Oct. 29. 1747. IN the Paper I did myſelf the Honour ſome time ſince to communicate to the *Royal Society*, I took notice, that, among the many other ſurpriſing Properties of Electricity, none was more remarkable, than that the electrical Power, accumulated in any non-electric Matter contained in a glaſs Phial, deſcribed upon its Exploſion a Circuit through any Line of Subſtances non-electrical in a conſidera-

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ble Degree; if one End thereof was in Contact with the external Surface of this Phial, and the other End upon the Explosion touched either the electrified Gun-barrel, to which the Phial in charging was usually connected, or the iron Hook always fitted therein. This Circuit, where the non-electric Substances, which happen to be between the Outside of the Phial and its Hook, conduct Electricity equally well, is always described in the shortest manner possible; but if they conduct differently, this Circuit is always formed through the best Conductor, how great soever its Length is, rather than through one which conducts not so well, though of much less Extent.

It has been found, that in proportion as Bodies are susceptible of having Electricity excited in them by Friction, in that Proportion they are less fit to conduct it to other Bodies; in consequence whereof, of all the Substances we are acquainted with, Metals conduct best the electrical Powers; for which Reason the Circuit before spoken of is formed through them the most readily. Water likewise is an admirable Conductor; for the electrical Power makes no Difference between Solids and Fluids as such, but only as they are non-electric Matter.

In order to give an Idea of what is understood by this Circuit, we will mention an Example or two, from which all the other may naturally be deduced. If a Person stands upon a dry wooden Floor with a coated Phial ever so highly charged in one of his Hands, and if another Person, without touching the first, stands but six Inches from him, and touches the iron Hook of the Phial, neither of them

are ſhocked; becauſe the Floor between them, tho' the Diſtance is ſo ſhort, will not conduct the Electricity ſufficiently quick. But if theſe two Perſons tread upon a Piece of Wire laid between them, they each of them feel the electrical Commotion in that Arm, which touches the Phial and Hook, and in that Foot which treads upon the Wire; the Wire here conducting the Electricity quick enough, which the dry Floor would not. The Circuit is here formed by the coated Phial, its Hook, ſo much of the Bodies of theſe two Perſons as formed a curve Line between the Wire, the Phial, and Hook, and the Wire between theſe Perſons. If theſe Perſons ſtand upon, or touch with any Part of their Bodies any Non-electrics, which readily conduct Electricity, the Circuit is completed, and the Effect is the ſame: And this is occaſion'd by the ſhort Space of Time, in which the loaded Phial is diſcharged, when any Matter of what kind ſoever readily conducting Electricity happens to be between the coated Phial and its Hook, and is ſo connected as to communicate with both upon the Diſcharge of the Phial.

Monſieur *le Mounier* the younger at *Paris*, in an Account tranſmitted to the *Royal Society*, takes notice of his feeling the Stroke of the electrified Phial along the Water of two of the Baſons of the *Thuilleries* (the Surface of one of which is about an Acre) by means of an iron Chain which lay upon the Ground, and was ſtretched round half their Circumference.

Upon theſe Conſiderations it was conjectured, as no Circuit had as yet been found large enough ſo to diſſipate the electrical Power as not to make it

 perceptible,

perceptible, that if the non-electrical Conductors were properly disposed, an Observer might be made sensible of the electrical Commotion quite across the River *Thames*, by the Communication of no other Medium than the Water of that River. But as perhaps, in what relates to Electricity less than in any other Part of Natural Philosophy, we should draw Conclusions but from the Facts themselves, it was determined to make the Experiment.

The making this Experiment drew on many others, and as the Gentlemen concerned flatter themselves that they were made with some Degree of Attention and Accuracy, they thought it not improper to lay a Detail of all the Operations relating thereto, before the *Royal Society*.

In order to try whether or no the electrical Commotion would be perceptible across the *Thames*, it was absolutely necessary that a Line of non-electric Matter, equal in Length to the Breadth of the River, should be laid over it so as to touch the Water thereof in no Part of its Length; and the Bridge at *Westminster* was thought the most proper for that Purpose, where the Water from Shore to Shore was somewhat more than 400 Yards.

Accordingly on *Tuesday July* 14, 1747. to see the Success and assist in making the Experiment, there met *Martin Folkes* Esq; President of the *Royal Society*, the Right Honourable the Earl *Stanhope*, *Richard Graham* Esq; *Nicholas Mann* Esq; and myself, with proper Persons to execute what was required of them in the various Parts of these Experiments.

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A Line of Wire was laid along the Bridge, not only through its whole Length, but likewiſe turning at the Abutments, reached down the ſtone-Steps on each Side of the River low enough for an Obſerver to dip into the Water an iron Rod held in his Hand. One of the Company then ſtood upon the Steps of the *Weſtminſter* Shore holding this Wire in his left Hand, and an iron Rod touching the Water in his right: On the Steps facing the former upon the *Surry* Shore, another of the Company took hold of the Wire with his right Hand, and graſped with his left a large Phial almoſt filled with Filings of Iron, coated with Sheet-Lead, and highly electrified by a glaſs Globe properly diſpoſed in a neighbouring Houſe. A third Obſerver ſtanding near the ſecond dipped an iron Rod held in his left Hand into the Water, and touching the iron Hook of the charged Phial with a Finger of his right Hand, the Electricity ſnapped, and its Commotion was felt by all the three Obſervers, but much more by thoſe upon the *Surry* Shore. The third Obſerver here was no otherwiſe neceſſary, than that the River being full, the Iron was not long enough to be fixed in the Mud upon the Shore, and therefore was in want of ſome Support. The Experiment was repeated ſeveral times, and the electrical Commotion felt acroſs the River; but the Gentlemen preſent being much moleſted in their Operations by a great Concourſe of People, who many times broke the conducting Wire, and otherwiſe greatly incommoded them, and the Evening growing too dark for the Obſervers on different Sides of the Water to ſee each other, they were prevented

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from diverſifying the Experiments, as was intended, and only conſider'd theſe Trials as a ſtill further Encouragement for them to proſecute the Inquiry at a more favourable Opportunity.

Early therefore on *Saturday* Morning *July* 18, there met upon *Weſtminſter-Bridge* the *Preſident* of the *Royal Society*, the Right Honourable the Lord *Charles Cavendiſh*, *Richard Graham* Eſq; Dr. *Bevis*, and myſelf, with proper Aſſiſtants. At the preceding Meeting, the electrical Machine's being placed at ſome Diſtance from the Water being found inconvenient, the following Alteration was made in the Diſpoſition of the *Apparatus*.

A Room up two Pair of Stairs in a commodious Houſe neareſt the Bridge on the *Surry* Shore was provided, in which was placed the electrical Machine with the Gun-barrel ſuſpended in ſilk Lines. From this Room, on account of its Height, the Signals on both Sides of the River were eaſily obſervable. The coated Phial before-mention'd with its iron Hook was placed upon the Seat of the Window of this Room, and communicated with the Gun-barrel by the means of a Piece of iron Wire. One Extremity of another Wire was likewiſe fixed into the Bottom of the leaden Coating of the Phial, whoſe other Extremity reached therefrom over the Bridge to the Steps upon the *Weſtminſter* Shore, the Body of the Wire being placed as much as poſſible upon the Parapet of the Bridge. One or more Obſervers took each other by the Hand, the firſt of which muſt neceſſarily take the Wire in his left Hand, and the laſt, upon the proper Signal given, either dip his right Hand into

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the Water, or (which makes the Posture more agreeable) a Rod of Metal held therein. Another Wire having no Communication with any of the former, was let down from the before-mention'd Room, and down the Steps upon the *Surry* Shore: One Extremity of this Wire was held in the Hand of an Observer standing upon these Steps, who dipped an iron Rod held in his other Hand into the Water: To the other Extremity of this Wire was fastened a short iron Rod, with which, when the elecfied Phial was sufficiently charged, and the Signal given, the Gun-barrel was to be touched.

The Gentlemen, by this Disposition of the *Apparatus*, proposed to examine principally these three Questions: First, whether or no the Observers standing on each Side of the River would perceive the electrical Commotion, each putting an iron Rod into the Water? Secondly, Whether or no the Observers on both Sides of the River would feel the electrical Commotion, when the Observer standing upon the *Westminster* Shore removed the iron Rod held in his Hand out of the Water? Thirdly, Whether or no the electrical Power was perceptible to the Observers on both Sides of the River, if the Observer upon the *Westminster* Shore dipped his Hand into a Pail of Water, which had no Communication with the Water of the *Thames*.

It was determined first, upon proper Signals, to discharge the electrified Phial in the manner before-mention'd, the Observers on each Side of the River holding the iron Rods in the Water, and this Experiment was to be repeated three times. This was attempted accordingly; and although the Observer

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on the *Surry* Shore was each time ſmartly ſtruck, the *Preſident* of the *Royal Society*, who obſerved with the utmoſt Attention upon the *Weſtminſter* Shore, gave the Signal that he felt nothing. The Company was ſurpriſed at this Want of Succeſs in the Experiment; but, upon examining the Wire, which was laid over the Bridge, it was found to have been broken by ſome Accident, after it had paſſed over about a fourth Part of the Bridge. The Wire being refitted, it was agreed to make the ſame Experiment ſix times more: This was done accordingly, and the electrical Commotion was felt each time by the Obſervers on both Sides of the Water, but much ſmarter by thoſe on the *Surry* Side. It was then thought proper to repeat this Experiment three times more upon the Signal's being given: but, in making the firſt of theſe, the Obſerver in the Room with the Machine, diſcharged the electrified Phial, before the Obſerver upon the *Surry* Shore had dipped his iron Rod into the Water, and therefore no Effect was perceived by the Obſerver on the oppoſite Shore. The electrified Phial therefore was again diſcharged three other times, and the Commotion felt by the Obſervers on both Sides of the River.

To examine the ſecond Queſtion, no other Alteration was neceſſary in the whole *Apparatus*, than that the Obſerver upon the *Weſtminſter* Shore ſhould not dip either his Hand, or the iron Rod held therein in the laſt Experiments, into the Water of the River. The electrified Phial then was diſcharged three times without its Effects being in the leaſt perceiv'd by the Obſervers upon the *Weſtminſter* Shore; thoſe indeed on that of *Surry* felt the Shock as before.

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In examining the third Queſtion, the *Apparatus* was in all other Reſpects the ſame as in the laſt; except that the Obſerver upon the *Weſtminſter* Shore had a Pail of Water placed upon a wooden Table, which ſtood upon the Stone Steps, and into which he was to put his right Hand upon the Signal's being given. This was accordingly done, and the electrified Phial being diſcharged three times, the electrical Commotion was felt as before by the Obſerver upon the *Surry* Shore; but not in the leaſt by him on the *Weſtminſter* Side, who held his Hand in the Pail of Water.

In all theſe Experiments, except in one before-mention'd, where the iron Rod was not in the Water, it was found, that whether the Obſervers on the *Weſtminſter* Shore, upon the Diſcharge of the electrified Phial, did or did not feel its Effects, they were always perceiv'd not only in the Arms of thoſe upon the *Surry* Shore, who formed a Line between the Extremity of the Wire there, and the Water of the River; but by any other Perſon, who ſtanding upon the Stone Steps, even where they were not wet, touched the Wire with his Hand. They were likewiſe felt by a Perſon upon the *Weſtminſter* Shore, ſtanding upon the wet Stone Steps, who did not form Part of the Line between the Extremity of the conducting Wire and the Water, otherwiſe than by touching the Wire with his Fingers.

As was before-mention'd, the Obſervers upon the *Weſtminſter* Shore did not feel the Effects of the diſcharged Phial near ſo ſtrong as thoſe on that of *Surry* in the firſt Set of theſe Experiments. When a Line was there form'd by the joining Hands of two or more Perſons, the firſt of which, on account

of the Situation, held the conducting Wire in his left Hand, and the last touched the Water with an iron Rod held in his right, the Effects were most sensible in the left Arm of him who held the Wire: They were indeed manifestly felt by them all; but this Feeling was not great enough to be called a Shock, but, as was very properly expressed by one of the Company, it resembled the Pulsation of a large Artery.

From the Examination of the first and second Questions it appeared, that the Observers upon the *Westminster* Shore were not sensible of the Effects of the Electricity, unless their Bodies described Part of the Circuit before spoken of; and this Circuit here consisted of Part of the Gun-barrel of the electrifying Machine, the Wire going from this Gun-barrel to the iron Hook, the Phial itself, the tail Wire of this coated Phial which reached therefrom across the Bridge and down the Steps on the *Westminster* Shore, the Line of Observers between this Wire and the iron Rod which dipp'd in the Water there, this iron Rod, a supposed Line of Water drawn quite across the *Thames*, the Observers with their iron Rod on the *Surry* Shore, the iron Wire going from the right Hand of the last of these up into the Room where the electrifying Machine was placed, and the short iron Rod to which one Extremity of this Wire was joined, and with which, in making the Explosion, the Gun-barrel was touched. The Length of this Circuit, through which the Electricity was propagated was, at least 800 Yards, more than 400 Yards of which was formed by the Stream of the River.

From the Examination of the third Question it appeared, that the electrical Commotion would not

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be felt from the Obſerver dipping his Hand in Water only, unleſs that Water was ſo diſpoſed as to become Part of the Circuit; and this Experiment was made, leſt the contrary might be ſurmiſed.

The Obſervers upon the *Weſtminſter* Shore not feeling the electrical Commotion equally ſtrong with thoſe of *Surry*, was judged to proceed from other Cauſes beſides that of Diſtance. For it muſt be conſider'd, that the conducting Wire was almoſt throughout its whole Length laid upon *Portland* Stone ſtanding in Water. This Stone, being in a great Degree non-electric, is of itſelf a Conductor of Electricity: And this Stone ſtanding in Water, no more of the Electricity was tranſmitted to the Obſervers on the *Weſtminſter* Shore than that Proportion, wherein Iron is more non-electric, and, conſequently, a better Conductor of Electricity than Stone. This was made more manifeſt, from obſerving that whether the conducting Wire upon the Bridge was broke or no, and, conſequently, whether the Obſervers upon the *Weſtminſter* Shore felt the electrical Commotion or no, not only the Obſervers upon the *Surry* Shore, who with their Wire form'd Part of the Line, felt the Shock in their Arms; but thoſe Perſons who only ſtood upon the Stone Steps there, and touched the Wire with their Fingers, felt the electrical Commotion in the Arm of that Hand which touched the Wire, and down their Legs. From whence, and from the Perſon before ſpoken of feeling the electrical Commotion ſtanding upon the wet Stone Steps of the *Weſtminſter* Shore, tho' not forming Part of the Line, but only touching the Wire with his Fingers, it was concluded, that, beſides the large Circuit before ſpoken of, there were

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formed ſeveral other ſubordinate Circuits between the ſame Steps of the *Surry* Shore, and the Bridge by means of the Water; whereby that Part of the electrical Power, felt by the Obſervers upon the *Surry* Side of the River, and not by thoſe on the *Weſtminſter* Side, was diſcharged.

Dr. *Bevis* having obſerved. and which was likewiſe tried here, that however well an electrified Phial was charged, its iron Hook would not fire the Vapours of warm Spirit of Wine held in a Spoon and applied thereto, if the Perſon who held the Phial, and he who held the Spoon did not take each other by the Hand, or have ſome other non-electrical Communication between them; it was therefore thought proper to try the Effects of Electricity upon ſome warm Spirit of Wine through the large Circuit before-mention'd. Accordingly the Obſervers being placed as before both upon the *Weſtminſter* and *Surry* Shores, no other Alteration was made in the before-mention'd *Apparatus*, than that the Wire which connected the Gun-barrel with the iron Hook of the coated Phial being laid aſide, the coated Phial itſelf was charged at the Gun-barrel, and then brought in the Hands of an Obſerver near the warm Spirits in the Spoon, which was placed upon the ſhort iron Rod before-mention'd, which was connected with the Wire which went to the Obſervers upon the *Surry* Shore. Upon preſenting properly the iron Hook of the charged Phial to the warm Spirit, it was inſtantly fired, and the electrical Commotion felt by the Obſervers on both Sides of the River.

It was then thought proper to try the Effects of the charged Phial upon the warm Spirit, when the

Wire

Wire was divided which was laid over the Bridge: Upon presenting the iron Hook to the Spirit, a sufficient Snap was given to the Spoon to fire the Spirit, but nothing so smart as in the former Experiment where the large Circuit was completed.

It was then tried, what the Effect would be upon the Spirit, if the charged Phial was divested of its long Wire which lay over the Bridge, and was only held in the Hand of an Observer; whilst the Spoon with warm Spirit was placed in Contact of the iron Rod before mention'd, to which the Wire was connected, which went to the Observers upon the *Surry* Shore; and the Spirit was fired with much the same Degree of Smartness as in the last Experiment.

In these and all the subsequent Operations, Wires were made use of to conduct the Electricity preferable to Chains, as it before by great Numbers of Experiments had been fully proved, that whatever Difference there was in the Bulk of the Conductor, that is to say, whether it were a small Wire, or a thick iron Bar, the electrical Strokes communicated thereby were equally strong: And it had been further observed, besides the Difficulty of procuring Chains of a requisite Length for the present Purposes, that the Stroke at the Gun-barrel, when the Electricity was conducted by a Chain, was *cæteris paribus* not so strong, as when that Power was conducted by a Wire. This was occasion'd by the Junctures of the Links of the Chain not being sufficiently close, which caused the Electricity in its Passage to snap and flash at the Junctures, where there was the least Separation; and these lesser Snappings in the whole Length of the Chain lessen'd the great one of the Gun-barrel.

Encouraged

Encouraged by the Succeſs of theſe Trials, the Gentlemen were deſirous of continuing their Inquiries, and of knowing whether or no the electrical Commotions were perceptible at a ſtill greater Diſtance. The *New River* near *Stoke Newington* was thought moſt convenient for that Purpoſe; as at the Bottom of that Town, the Twinings of the River are ſo circumſtanced, that from a Place which we will call *A* to another *B*, the Diſtance by Land is about 800 Feet, but the Courſe of the River is near 2000. From *A* to another Place, which we will call *C*, in a right Line is 2800 Feet, but the Courſe of the Water is near 8000 Feet.

Accordingly, on *Friday July* 24. 1747 there met at *Stoke-Newington* the *Preſident* of the *Royal Society*, the Right Honourable the Lord *Charles Cavendiſh*, the Rev. Mr. *Birch*, *James Burrow* Eſq; *Peter Daval* Eſq; Mr. *George Graham*, *Wm. Jones* Eſq; *James Lever* Eſq; Mr. *Newcome*, *Charles Stanhope* Eſq; Mr. *Trembley* and myſelf, who were of the *Royal Society*, and Dr. *Bevis*. To this Gentleman the Company were much obliged, not only for his great Readineſs in aſſiſting in all the Operations, but likewiſe for the Uſe of his electrifying Machine, which from its Size was conveniently portable. This Machine was now placed in a Room up one Pair of Stairs in a Houſe near *A*, and the Signals from thence might eaſily be perceived by the Obſervers both at *B* and *C*.

It was propoſed, firſt to try the electrical Commotion by the ſame Obſervers as at *Weſtminſter-Bridge*, from *A* to *B*, the Diſtance as before-men-

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tion'd being about 800 Feet by Land, and 2000 by Water, in order if possible to determine the Difference of the Strength of the Electricity felt there, and at the Stone-Bridge at *Westminster*; the Difference of the Length of the 2 Circuits being about 400 Feet in Favour of that of the new River.

To make the Experiment, an iron Wire was fastened to the Coating of the glass Phial before-mention'd, and conducted from one of the Windows of the Room over the new River without touching the Water; and from thence to *B*, laying in its whole Length upon the Grass in the Meadows, except where it passed over a Hedge. At *B*, when the Explosion was to be made, one or more Observers were to take the Extremity of this Wire in one Hand, and touch the Water of the River as before with an iron Rod held in the other. Another Wire was let down from the other Window of the Room; one Extremity of which was joined to the short iron Rod mention'd in the former Experiments, the other was held in the Hand of an Observer at *A*, whose other Hand held an iron Rod dipp'd into the River.

It was absolutely necessary that these Wires should touch each other in no Part of their Length, otherwise the before-mention'd Circuit would upon the Explosion be completed from their first Contact.

When every thing was thus disposed, and the Signals given, the charged Phial was exploded eight times, and the electrical Commotion every time smartly felt by the Observers both at *A* and *B*. Whether the Line of Observers at *B* consisted of one or more, they were always struck, and that more sharply than at *Westminster-Bridge* under the

same

ſame Circumſtances. One of the Obſervers, taking the Wire in his Hand without having any Communication either with any of the other Gentlemen or the Water of the River, felt the Shock in his Feet.

It was then thought proper to make right Exploſions without any other Alteration in the *Apparatus* than that the Obſervers at *B*, ſhould ſtand in the Meadow at ſome Diſtance from the Water, without having any Communication therewith other than that furniſhed by the Ground. This was accordingly done, and the Stroke felt little if at all leſs than thoſe laſt-mention'd. But the electrical Strokes being felt ſmartly at the Diſtance of at leaſt 20 Feet from the Water occaſion'd a very perplexing Difficulty, as it was impoſſible by this Experiment to determine with any Certainty, whether or no the electrical Circuit was formed throughout the Windings of the River, or much ſhorter by the Ground of the Meadows. The Experiment plainly ſhewed that the Meadow-Ground with the Graſs thereon conducted the Electricity better than Stone; as it muſt be remember'd, that the Obſervers upon the Stone Steps upon the *Weſtminſter* Shore felt not in the leaſt degree the electrical Commotion, when their iron Rod was not in the Water, and themſelves ſtood upon the dry Stone Steps. But this Effect was ſuppoſed to be owing to the Meadow Ground here being encompaſſed on two Sides by the *New River*, and on the other by a wet Ditch, by both which it was generally well moiſten'd. To ſolve therefore this Difficulty a Series of Experiments were executed, of which hereafter.

The Gentlemen then determin'd to examine whether the electrical Commotions were perceptible from

Commotions were perceptible from *A* to *C*; a Diſtance not leſs than 2800 Feet by Land, and near 8000 by Water.

To execute this, to the former Wire, which was already conducted to *B*, another was added, which there croſſed the River without touching the Water; and reached almoſt to *C*, where the firſt of a Line of Gentlemen held as before the Wire in one Hand, and the laſt dipp'd the Iron into the Water. The Wire from the Machine to *A* was as before. Upon the Signal's being given, the charged Phial was exploded ten times, and its Effects plainly though but faintly perceived each time by ſome or other of the Obſervers, but never by them all. The electrical Commotion was always felt by that Obſerver, who held the Extremity of the Wire, but never by him who held the iron Rod in the Water. It was in one Experiment felt by the Obſerver who held the Wire, not felt by the next who held the Hand of the former, and yet plainly perceived by the third who joined the ſecond. Thoſe who did not themſelves feel the electrical Commotion here, did as at *B* ſee the involuntary Motions of thoſe who did. The Obſervers at *A* felt the Shocks in the ſame Degree, whether the other Obſervers were ſtation'd at *B* or *C*.

This Experiment further demonſtrates the Diſtance to which the electrical Power may be conveyed: but the ſame Difficulty occurs here as in the laſt; to wit, whether the Circuit was compleated by the Ground, or by the Water of the River?

Theſe ſame Operations, which ſhewed at how great a Diſtance the electrical Commotion was per-

ceptible, ſolved likewiſe three Queſtions of a ſubordinate Nature.

Firſt, whether or no, *cæteris paribus*, any Difference occurred in the Succeſs of the Experiment, if the long Wire, inſtead of being joined to the Coating of the Phial, was faſten'd to the ſhort iron Rod, which upon touching the Gun-barrel occaſion'd the Exploſion; and if the ſhort Wire, which only went to the Obſerver at *A*, a Diſtance from the Machine not more than 30 Feet, was joined to the Coating of the Phial? Upon Trial no Difference * was found.

Secondly, Whether or no, *cæteris paribus*, any Difference in the electrical Commotion would be perceived, when that Power paſſes through the Arms of two Obſervers, whoſe Bodies made Part of the Circuit, ſtanding in the Room near the electrifying Machine; one of which takes the Extremity of the Wire that goes to the Obſerver at *A* in one Hand, and touches the Gun-barrel with the ſhort iron Rod held in his other Hand? The other Obſerver takes the Extremity of the Wire which goes to *B* or *C* in one Hand, and touches the Coating of the charged Phial with his other. In ſeveral Trials, where each of theſe Obſervers frequently changed Stations, no Difference in point of Strength was obſerved in the electrical Commotion.

Thirdly,

* No Difference is obſerved, when the electrical Circuit is propagated through Subſtances which readily conduct Electricity; if they conduct it in a leſs Degree, the electrical Commotion is moſt perceptible to the Obſerver, who holds the Wire, which comes from the charged Phial.

Thirdly, Whether or no theſe two Obſervers laſt-mention'd received the Shock at the ſame time? They were ſeen to be both convulſed in the ſame Inſtant.

July 28. 1747, there met again at the ſame Place, to proceed further in theſe Inquiries, the *Preſident* of the *Royal Society*, the Right Honourable the Lord *Charles Cavendiſh*, the Reverend Mr. *Birch*, Sir *Francis Daſhwood* Baronet, *Peter Daval* Eſq; Mr. *Ellicott*, Mr. *George Graham*, *Richard Graham* Eſq; Mr. *Robins*, Mr. *Short*, Dr. *Wilbraham*, and myſelf, who were of the *Royal Society*, and Dr. *Bevis*.

The electrical Commotion was firſt tried from *A* to *B* before-mention'd, the iron Wire in its whole Length being ſupported, without any-where touching the Ground, by dry Sticks placed at proper Intervals of about three Feet in Height. The Obſervers both at *A* and *B* ſtood upon Originally-Electrics, and, upon the Signal, dipped their iron Rods into the Water. Upon diſcharging the Phial, which was ſeveral times done, they were both very much ſhocked, much more ſo than when the conducting Wires lay upon the Ground, and the Obſervers ſtood thereon, as in the former Experiments. The ſame Experiment was tried with the Obſerver at *A*, inſtead of the iron Rod, dipping a narrow Slab of *Portland* Stone into the Water of about three Feet and a half in Length; when the Shock was felt, but not ſo ſevere as through the iron Rod. This demonſtrated, as was before ſuggeſted, why the electrical Commotion was not felt ſtronger by the Obſervers upon the Weſtern Shore of the *Weſtminſter-*

Bridge; *viz.* that *Portland* Stone ſtanding in Water will conduct Electricity very conſiderably.

The Gentlemen then tried what would be the Effect, if the Obſerver at *B* ſtood upon a Cake of Wax holding the Wire as before, and touched the Ground of the Meadow with his iron Rod at leaſt 150 Feet from the Water; and if the Obſerver uſually placed near the River at *A*, had his Wire carried 150 Feet over the River as the former, ſtood upon an Originally-Electric, and touched the Ground with his iron Rod. Upon the Exploſion of the charged Phial, which was ſeveral times done, both the Obſervers were ſmartly ſtruck: This demonſtrated, that in theſe Inſtances the moiſt Ground of the Meadows made Part of the Circuit. The Obſervers were diſtant from each other about 500 Feet.

The Obſervers then, ſtation'd as in the laſt Experiment, ſtood upon the wax Cakes as before, without touching the Ground with the iron Rods, or any Part of their Bodies, and the charged Phial was exploded four times. Theſe were not at all felt by the Obſerver next to *B*, and without the greateſt Attention would not have been perceived by him next to *A*; and then only in ſome of the Trials, the Feeling of the Electricity was like that of a ſmall Pulſe between the Finger and Thumb of that Hand which held the Wire. The loaded Phial was again diſcharged four times more, without any other Alteration in the Diſpoſition of the *Apparatus* than that the Obſerver next to *B* ſtood upon the Ground; when the electrical Commotion was perceived by that Obſerver, though not ſo ſharp as when the other Obſerver at the ſame time ſtood upon

upon the Ground. The Obſerver next to *A* felt the Tingling between his Finger and Thumb as before.

The Gentlemen were deſirous of trying the electrical Commotion at a ſtill greater Diſtance than any of the former through the Water, and where, at the ſame time by altering the Diſpoſition of the *Apparatus*, it might be tried, whether or no that Power would be perceptible through the dry Ground *only* at a conſiderable Diſtance. *Highbury* Barn beyound *Iſlington* was thought a convenient Place for this Purpoſe, as it was ſituated upon a Hill nearly in a Line, and almoſt equidiſtant from two Stations upon the *New River*, ſomewhat more than a Mile aſunder by Land, though following the Courſe of that River their Diſtance from each other was two Miles. The Hill between theſe Stations was of a gravelly Soil; which, from the late Continuance of hot Weather without Rain, was dry, full of Cracks, and conſequently was as proper to determine whether or no the Electricity would be conducted by dry Ground to any great Diſtance, as could be deſired. This hitherto had not been attempted; the Meadows in the Inſtances before quoted conducting the Electricity was ſuppoſed to be owing to the Moiſture of the Ground. The Streets of *London*, when very dry, had been found to conduct it ſtrongly about forty Yards, and the dry Road at *Newington* about the ſame Diſtance. Accordingly, on *Wedneſday*, *Aug*. 5. 1747. there met at *Highbury-Barn* the Right Honourable the Lord *Charles Cavendiſh*, the Reverend Mr. *Birch*, Mr. *George Graham*, *Richard Graham* Eſq; *N. Mann* Eſq; Mr.

Short,

Short, *Daniel Wray* Esq; and myself, who were of the *Royal Society*, and Dr. *Bevis*.

The electrifying Machine being placed up one Pair of the Stairs in the House at *Highbury-Barn*, a Wire from the coated Phial was conducted upon dry Sticks as before to that Station by the Side of the *New River*, which was to the Northward of the House. The Length of this Wire was 3 Furlongs and 6 Chains, or 2376 Feet. Another Wire fasten'd to the iron Bar, with which, in making the Explosion, the Gun-barrel was touched, was conducted in like manner to the Station upon the *New River* to the Southward of the House. The Length of this Wire was 4 Furlongs 5 Chains and 2 Poles, or 3003 Feet. The Length of both Wires, exclusive of their Turnings round the Sticks, was 1 Mile 1 Chain and 2 Poles, or 5379 Feet. For the more conveniently describing the Experiments made here, we will call the Station to the Northward *D*, and the other *E*.

At this Distance the Gentlemen proposed to try, first, Whether or no the electrical Commotion was perceptible, if both the Observers at *D* and *E*, supported by Originally-Electrics, touched the conducting Wire with one Hand, and the Water of the *New River* with an iron Rod held in the other? Secondly, Whether or no that Commotion was perceptible, if the Observer at *E*, being in all respects as before, the Observer at *D*, standing upon Wax, took his Rod out of the Water? Thirdly, Whether or no that Commotion was perceptible to both Observers, if the Observer at *D* was placed upon Wax,

and

and touched the Ground with his iron Rod in a dry gravelly Field at leaſt 300 Yards from the Water?

As from the Situation of the Ground, Trees, *&c.* neither of the Stations could be ſeen by each other, or by the Obſerver at the electrifying Machine, it was agreed to diſcharge a Gun as a Signal to get ready, and to do the ſame, as near as might be, half a Minute before each Exploſion.

In theſe Experiments, as well as the former, the coated Phial was each time charged as high as it could be; ſo that if the Difference of the Shock to the Obſervers was conſiderable, it was owing to other Cauſes more than to the Phial's being differently electrified.

To try the firſt Propoſition, eight Exploſions were made with the Obſervers at *D* and *E*, touching the Water, and ſtanding upon Wax, with their iron Rods in the Water. The firſt two of theſe were felt but weakly by the Obſerver at *D*; but in the other ſix he was ſtrongly ſhocked. The Obſerver at *E* felt nothing of the firſt ſix Exploſions; when, upon Examination, the Wire was found broken by ſome Accident; but this Obſerver was ſtrongly ſhocked by the two laſt. The Obſerver at *D* being ſhocked in four of theſe Exploſions, while in theſe four the Obſerver at *E* felt nothing, was owing to the Circuits being formed by the Ground between the Obſerver at *D* and the broken Wire. Upon account of the Wire's being broken, the Gentlemen tried three more Exploſions, when the Obſervers at both Stations felt the electrical Shock.

To try the ſecond Propoſition, four Exploſions were made with the Obſerver at *D* ſtanding upon

an Originally-Electric, and taking his iron Rod out of the Water, the Obſerver at *E* as before. In each of theſe the Obſerver at *D* felt a ſmall Pulſation between his Finger and Thumb of that Hand, which held the Wire. The Obſerver at *E* felt each of theſe as ſtrong as before. This being different from the Obſervations made in the Experiments of the laſt Trials at our former Stations *A* and *B*, and many others; where *B* in the ſame Circumſtances with *E* here felt the electrical Commotion only in a ſlight Degree, was owing, as we were afterwards informed, to the impertinent Curioſity of the Servants of the Gentlemen, and other voluntary Obſervers, who, by touching the Wire which went from the coated Phial to the Obſerver at *D*, felt the Shock in their Arms and Ankles, and formed ſubordinate Circuits to *E*. The preventing theſe People from touching the Wires, was impoſſible; as great Part of them could be ſeen neither by the Obſervers at the Stations, nor by thoſe at the Houſe, and their being more than a Mile long.

The four other Exploſions were made without any other Alteration in the *Apparatus*, than that the Obſerver at *D* ſtood upon the Ground about four Yards from the Water without any Communication therewith. The Obſerver at *E* felt the Shocks in his Arms as before; but the Obſerver at *D* ſtanding upon the Ground was ſhocked in the Elbow and Wriſt of that Arm which held the Wire, and in both his Ankles.

To try the third Propoſition, eight Exploſions were made with the Obſerver at *D* ſtanding upon an Originally-Electric with his Rod in the Water of

the

the River as before; but the Obſerver at *E* was placed in a dry gravelly Field about 300 Yards nearer the Machine than his laſt Station, and about 100 Yards diſtant from the River. He there ſtood upon the Wax, holding the conducting Wire in one Hand, and touched the Ground with an iron Rod held in the other. The Shock was each time felt by the Obſerver at *D*, but ſenſibly weaker than in the former Trials; but the Obſerver at *E* felt them all equally ſtrong with the former; the four firſt in his Arms, when he ſtood upon the Wax, and touched the Ground with his iron Rod; the other four in his Arm and Ankles, when he ſtood upon the Ground without the iron Rod.

In ſome of theſe Experiments, the Obſervers at *D* felt a Tingling as ſoon as they laid hold of the conducting Wire. This was conjectured to be owing to the Electricity, which conſtantly runs off while the coated Phial is filling, and preferably by the Wire, as the beſt Conductor.

From the Severity of the Shock, the Gentlemen, in ſome of theſe Trials, did not chooſe to have the Electricity paſs through their Bodies: But, as it was neceſſary for them to be ſenſible of the different Degrees of the electrical Commotions, they bound the conducting Wire round one of their Thumbs, and touched the iron Rod with the Fore-finger of the ſame Hand; when the electrical Commotion was felt only in ſo much of the Finger and Thumb of that Hand, as completed the Circuit.

By the Experiments of this Day, the Gentlemen were ſatisfied, that the dry gravelly Ground conducted the Electricity as ſtrongly as Water; which, though

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otherwise at first conjectured, they now found not to be necessary to convey that Power to great Distances; as well as that, from Difference of Distance only, the Force of the electrical Commotion was very little if at all impaired. They were convinced of the Truth of the first of these Facts, not only from both Observers feeling the electrical Commotion in the eight last Experiments, when the Observer at *E* was at such a Distance from the Water, but also from the Observer at *D* feeling the Shock so strong in four of the first six Explosions, when the conducting Wire to *E* being broke at about 100 Yards Distance from the House, that Observer felt nothing.

In this last Instance the Circuit was formed from the Phial by the Observer at *D* and his Wire, a Line of Ground which reached from the Station at *D* to the broken Wire that lay upon the Ground, and so much of this Wire as reached to the short iron Rod, which touched the Gun-barrel in making the Explosions. This induced the Gentlemen to conclude (as from many Experiments it was manifest, that when the intervening Substances conduct Electricity equally well, the Circuit was performed in the shortest manner possible), that when the Observers holding their iron Rods in the River at *D* and *E* were both shocked, the Electricity was not conveyed by the Water of the River, being two Miles in Length, but by Land, where the Distance was only one Mile; in which Space that Power must necessarily pass over the *New River* twice, through several Gravel-Pits, and a large Stubble-Field. So that, admitting the Electricity did not

follow the Tract of the River, the Circuit from *D* to *E* was at leaſt two Miles; *viz.* ſomewhat more than one Mile of Wire, which conducted the Electricity from the Houſe to the Stations, and another Mile of Ground, the ſhorteſt Diſtance between thoſe Stations. The ſame Inference was now drawn with regard to the Experiments at *A*, *B*, and *C*, in the *New River* before recited; *viz.* that as in all of them the Diſtance between the Obſervers was much greater by Water than by Land, the Electricity paſſed by Land from one Obſerver to the other, and not by Water.

From the Shocks which the Gentlemen received in their Bodies, when the electrical Power was conducted upon dry Sticks, they were of Opinion, that from Difference of Diſtance ſimply conſider'd, as far as they had yet experienced, the Force thereof was very little if at all impaired. When they ſtood upon Originally-Electrics, and touched the Water or Ground with an iron Rod, the electrical Commotion was always felt in their Arms and Wriſts: When they ſtood upon the Ground, and touched either the Water or Ground with their iron Rods, they felt the Shock in their Elbows, Wriſts, and Ankles: When they ſtood upon the Ground without the Rod, the Shock was always in the Elbow and Wriſt of that Hand, which held the conducting Wire, and in both Ankles. The Obſervers here being ſenſible of the electrical Commotion in different Parts of their Bodies, was owing in the firſt Inſtance to the Whole of its paſſing (becauſe the Obſerver ſtood upon Wax) through their Arms, and through the iron Rod: In the ſecond, when they ſtood upon the Ground, the

the Electricity passed both through their Legs, and thro' the Iron: In the third, when they stood upon the Ground without either Wax or Rod, the Electricity directed its Way through one Arm, and through both Legs to complete the Circuit.

The Gentlemen were desirous of closing the present Inquiry, by examining not only whether or no the electrical Commotions were perceptible at double the Distance of the last Experiments in Ground perfectly dry, and where no Water was near; but also, if possible, to distinguish the respective Velocities of Electricity and Sound. To execute this, required the whole Sagacity and Address of the Gentlemen concerned; for they had met with very great Difficulties in the last Day's Operations, where the Wire was conducted but little more than a Mile; all which could not but be greatly augmented by doubling that Distance; because it was necessary, that the House, wherein the electrifying Machine was placed, should be visible at least at one of the Stations; and that the Space between that House and the Stations, through which the Wire was conducted, should be very little intersected by Hedges, Roads, or Foot-paths; neither should the Wire in this Space be subject to be disturbed by the Horses or Cattle, which were grazing; nor ought it to touch in its Passage the Trees or any other Vegetables, which at this Season of the Year were every-where luxuriant. To find a Place within a convenient Distance of *London* with these Requisites was not very easy; but at last, *Shooters Hill* was pitched upon, as the most convenient.

As

As only one Shower of Rain had fallen during the preceding five Weeks, the Ground could not but be very dry; and as no Water was near, if the electrical Commotion was felt by the Obſervers at the Stations, it might be ſafely concluded, that Water had no Share in conducting it.

Auguſt 14. 1747. there met at *Shooters Hill* for this Purpoſe, the Rev. Mr. *Birch*, the Rev. Mr. Profeſſor *Bradley*, *Peter Daval* Eſq; Mr. *George Graham*, *R. Graham* Eſq; Mr. *Nourſe*, *George Lewis Scott* Eſq; Mr. *Short*, *Charles Stanhope* Eſq; and myſelf, who were of the *Royal Society*, and Dr. *Bevis*.

It was here determin'd (as the Gentlemen were ſatisfied from many of the former Trials, that if, when the coated Phial was diſcharged, the Obſervers at the Stations ſtood upon Originally-Electrics, and touched neither Water nor Ground with iron Rods, or any Part of their Bodies, the electrical Commotion would be ſcarcely perceptible) to make twelve Exploſions of the coated Phial, with an Obſerver placed at the ſeven Mile-Stone, and another at the nine Mile-Stone, both ſtanding upon Wax, and touching the Ground with an iron Rod. This Number of Exploſions was thought more neceſſary, as the Obſervers at theſe Stations were not only to examine whether or no the Electricity would be propagated to ſo great a Diſtance; but if were, the Obſerver at the ſeven Mile-Stone was by a ſecond Watch to take notice of the Time lapſed between feeling the electrical Commotion, and hearing the Report of a Gun fired near the Machine, as cloſe as might be to the Inſtant of making the

Exploſion:

Exploſion: And therefore, to examine this Matter with the requiſite Exactneſs, this Number of Exploſions ſhould be made.

To execute this, the electrifying Machine was placed up one Pair of Stairs in a Houſe upon the Weſt Side of *Shooters Hill*; and a Wire from the ſhort iron Rod, with which the Gun-barrel was touched in making the Exploſions was conducted upon dry Sticks as before into a Field near the ſeven Mile-Stone. The Length of this Wire, excluſive of its Turnings round the Sticks, was a Mile, a Quarter and eight Poles, or 6732 Feet. In great Part of this Space it was found very difficult to ſupport the Wire, on account of our ſcarcely being able to fix the Sticks in the ſtrong Gravel there almoſt without any Cover of Soil; nor could the Wire in ſome Places be prevented from touching the Brambles and Buſhes, nor in one Field the ripe Barley.

Another Wire was likewiſe conducted upon Sticks from the coated Phial to the nine Mile-Stone. In this Space, the Soil being a ſtrong Clay, the Wire was very well ſecured, and in its whole Length did not touch the Buſhes. The Length of this Wire was 3868 Feet. As much as the Place, where the Obſervers were ſtation'd in a Corn-Field, was nearer the Machine than the ſeven Mile-Stone, ſo much were the other Obſervers placed beyond the nine Mile-Stone, that their Diſtance from each other might be two Miles. The forty Feet of Wire in theſe two Meaſures exceeding two Miles, was what connected the ſhort iron Rod before-mention'd, and the coated Phial, with their reſpective conducting Wires.

The

The Obſervers being placed at their reſpective Stations, the Obſerver at the Machine proceeded in making the Exploſions of the coated Phial; he having before placed an Aſſiſtant exactly in his View before the Window of the Houſe, who, upon the Word of Command, was to diſcharge a Musket. As ſoon as ever the Flaſh was ſeen to come from the Mouth of the Gun, the Obſerver diſcharged the electrified Phial. When eight Exploſions had been made, a Servant was ſent from the Gentlemen at the ſeven Mile-Stone giving an Account of the Wire's being broken, and the Sticks thrown down by a Man riding through them; that the Obſervers there had felt nothing; and deſired, as by this time the Wire was replaced, that we ſhould begin again. This was complied with, and twelve other Exploſions made without further Moleſtation.

Not only the firſt eight, but eleven of the laſt twelve very ſtrongly ſhocked the Obſervers at the nine Mile-Stone: At the twelfth Exploſion the Obſerver on purpoſe ſtood upon the Wax without touching the Ground with his iron Rod, or any Part of his Body; and only felt a ſlight Tingling in his Finger and Thumb that held the Wire. In another of theſe Experiments, as the Gentlemen here were ſatisfied in their own Perſons of the Strength of the electrical Commotion, they indulged two Country Fellows, who were By-ſtanders, with feeling one: Theſe two with four of the Gentlemen formed a Chain, the firſt of them taking hold of the Extremity of the Wire with one of his Hands. They all ſtood upon the Ground, and made no Uſe

of

of the iron Rod. Upon the Explosion they were all so strongly shocked in their Arms and Ankles, that the Countrymen could by no means be prevailed upon to try the Experiment again. Why, in the first eight Explosions, the Observers here were sensible of the electrical Commotion, when the Observers at the other Station felt nothing, was explained in the former Experiments. The Observers at this Station, from their Situation under the Hill, and from what Wind there was being against it, never heard the Report of the Gun.

Though the Observers near the seven Mile-Stone from the breaking of their Wire, were not sensible of the eight first Explosions of the charged Phial, they felt the other twelve. This demonstrated to the Satisfaction of the Gentlemen concerned, that the Circuit here formed by the Electricity was four Miles; *viz.* two Miles of Wire, and two Miles of Ground, the Space between the Extremities of that Wire. A Distance without Trial too great to be credited! How much further the electrical Commotion will be perceptible, future Observations can only determine.

The electrical Commotion by the Observers near the seven Mile-Stone was but slightly felt; nor could it be otherwise expected, the Wire in many Parts of its Length touching, as was before mention'd, the moist Vegetables; which, in as many Places as they were touched, formed subordinate Circuits. We find, in all other Instances, that the whole Quantity of Electricity, accumulated in the coated Phial, is felt equally through the whole Circuit, when

every

every Part thereof is in a great degree non-electric; so here the whole Quantity, or nearly so †, determined that Way, was felt by the Observers at the nine Mile-Stone; whilst those at the other Station felt so much of their Quantity only, as did not go through the Vegetables; that is, that Proportion only in which Iron is a greater Non-electric than the Vegetables.

Tho' the electrical Commotions, felt by the Observers near the seven Mile-Stone, were not strong; they were equally conclusive in shewing the Difference between the respective Velocities of Electricity and Sound.

The Space through which Sound is propagated in a given Time, has been very differently estimated by the Authors, who have wrote concerning this Subject. *Roberval* gives it at the Rate of 560 Feet in a Second; *Gassendus*, at 1473; *Mersenne* at 1474; *Du Hamel*, in the History of the *Academy of Sciences* at *Paris*, at 1172; the *Academy del Cimento*, at 1185; *Boyle* at 1200; *Roberts* at 1300; *Walker* at 1338; Sir *Isaac Newton* at 968; Dr. *Derham*, in whose Measure Mr. *Flamsteed* and Dr. *Halley* acquiesced, at 1142. But by the Accounts since published by M. *Cassini de Thury* in the Memoirs of the *Royal Academy* of *Sciences* at *Paris* for the Year 1738. where Cannon were fired at various as well as great Distances, under great Variety of Weather, Wind, and other Circumstances, and

† The Author of this Paper, from a great Variety of Experiments, is of Opinion; that in this and the like Dispositions of the *Apparatus*, the electrical Power, accumulated in the Matter contained in the coated Phial, is directed upon the Explosion thereof towards both Observers at the same Instant.

and where the Meaſures of the different Places had been ſettled with the utmoſt Exactneſs, Sound was propagated at a *Medium* at the Rate only of 1038 *French* Feet in a Second. The *French* Foot exceeds the *Engliſh* by ſeven Lines and a half, or is as 107 to 114: And conſequently 1038 *French* Feet are equal to 1106 *Engliſh* Feet. The Difference therefore of the Meaſures of Dr. *Derham* and M. *Caſſini* is $\frac{34\ \textit{French}}{36\ \textit{Engliſh}}$ Feet in a Second.† According to this laſt Meaſure, the Velocity of Sound, when the * Wind is ſtill, is ſettled at the Rate of a Mile, or 5280 *Engliſh* Feet in $4''\frac{77}{100}$.

To return to our Purpoſe; the Length of the conducting Wire from the Machine to the Obſervers near the ſeven Mile-Stone was (as has been before-mention'd) a Mile, a Quarter, and 8 Poles, or 6732 Feet: The Length of that to the nine Mile-Stone, 3868 Feet. The firſt of theſe Meaſures only was made uſe of in the preſent Operations concerning the Velocity of Electricity. In twelve Diſcharges of the coated Phial, which were felt by Mr. *George Graham*, Mr. *Short*, and *Charles Stanhope* Eſq; the Obſervers near the ſeven Mile-Stone, and who, by a ſecond Watch of Mr. *Graham*'s, meaſured the Time

† M. *Caſſini de Thury* afterwards meaſured the Velocity of Sound at *Aiguemortes* in *Languedoc*, and found the Obſervations there from thoſe made about *Paris* vary only half a Toiſe in a Second. See *Mem. de l' Acad. Royale des Sciences, pour l'année* 1739, *p.* 126.

* Dr. *Derham* found, that when Sound was carried againſt the Wind, not only its Diſtance but its Velocity was leſſen'd; and in M. *Caſſini*'s Memoir, there is an Experiment, where Sound being carried againſt the Wind, which then blew very ſtrong, was retarded near a twelfth Part of the uſual Time in its Progreſs.

Time between feeling the electrical Commotion, and hearing the Report of the Gun, with the utmost Attention and Exactness; the Time, I say, between feeling the electrical Commotion, and hearing the Report of the Gun, was, at a *Medium*, 5 Seconds and a Quarter, or $5'' \frac{250}{1000}$. And as the Gun was distant from these Observers 6732 Feet, it follows, from the Experiments, which have been made on the Velocity of Sound, that the real Instant of the Discharge of the Gun preceded that of the Observers hearing its Report, at this time when the Strength of the Wind was not so great as to enter into the Computation, $6'' \frac{.087}{.1000}$; or preceded the Instant when the electrical Commotion was felt only $0''. \frac{837}{1000}$. But this Instant was, from the Nature of the Experiment, necessarily prior to that of the electrical Explosion, which was not made till the Fire of the Gun was actually seen; and therefore the Time between the making of that Explosion, and its being actually felt by the Observer, which must have been less than $0''. \frac{837}{1000}$, was really so small, as not to fall under any certain Observation, when it is to be distinguished from that, which must of Necessity be lost, between the Firing of the Gun, and the electrical Explosion itself.

In all the Experiments, where the Circuit was formed to any considerable Length, though the coated Phial was very well charged, the Snap at the Gun-barrel, upon the Explosion, was not near so loud as when the Circuit is formed in a Room; so that a By-stander, though versed in these Operations, from seeing the Flash, and hearing the Report, would imagine the Stroke at the Ends of the conducting Wire to be very slight; the contrary whereof,

when the Wire has been properly conducted, has always happen'd.

From a Review of theſe Experiments, the following Obſervations may be deduced.

I. That, in all the preceding Operations, when the Wires have been properly conducted, the electrical Commotions from the charged Phial have been very conſiderable only, when the Obſervers at the Extremities of the Wire have touched ſome Subſtance readily conducting Electricity with ſome Part of their Bodies,

II. That the electrical Commotion is always felt moſt ſenſibly in thoſe Parts of the Bodies of the Obſervers, which are between the conducting Wires, and the neareſt and the moſt non-electric Subſtance; or in other Words, ſo much of their Bodies, as comes within the electrical Circuit.

III. That, upon theſe Conſiderations, we infer, that the electrical Power is conducted between theſe Obſervers by any non electric Subſtances, which happen to be ſituated between them, and contribute to form the electrical Circuit.

IV. That the electrical Commotion has been perceptible to two or more Obſervers at conſiderable Diſtances from each other, even as far as two Miles.

V. That when the Obſervers have been ſhocked at the End of two Miles of Wire, we infer, that the electrical Circuit is four Miles; *viz.* two Miles of Wire, and the Space of two Miles of the non-electric

Matter

Matter between the Obſervers, whether it be Water, Earth, or both.

VI. That the electrical Commotion is equally ſtrong, whether it is conducted by Water or dry Ground.

VII. That if the Wires, between the electrifying Machine and the Obſervers, are conducted upon dry Sticks, or other Subſtances non-electric in a ſlight Degree only, the Effects of the electrical Power are much greater than when the Wires in their Progreſs touch the Ground, moiſt Vegetables, or other Subſtances in a great Degree non-electric.

VIII. That by comparing the reſpective Velocities of Electricity and Sound; that of Electricity, in any of the Diſtances yet experienced, is nearly inſtantaneous.

I ſhall conclude this Paper with obſerving, that it was thought convenient to lay a Detail of all the Operations relating to theſe Experiments before the *Society*; in conſequence of which the Gentlemen may make themſelves Judges, how far the Deductions here recited are warrantable from the Experiments.

* The Gentlemen concerned were deſirous, if poſſible, of aſcertaining the abſolute Velocity of Electricity

* Theſe Experiments to meaſure the abſolute Velocity of Electricity were made whilſt this Paper was at the Preſs, but as they had ſo near a Relation to the Experiments made the preceding Year, it was thought proper to inſert them here.

Electricity at a certain Distance; because, although last Year, in measuring the respective Velocities of Electricity and Sound, the Time of its Progress was found to be very little, yet we were desirous of knowing, small as that Time was, whether it was measurable; and I had thought of a Method for this Purpose.

Accordingly, *August* 5. 1748. there met at *Shooter's Hill* for this Purpose the *President* of the *Royal Society*, the Rev. Mr. *Birch*, the Rev. Mr. Professor *Bradley*, *James Burrow* Esq; Mr. *Ellicot*, Mr. *George Graham*, *Richard Graham* Esq; the Rev. Mr. *Lawrie*, *Charles Stanhope* Esq; and myself, who were of the *Royal Society*, Dr. *Bevis*, and Mr. *Grischow* a Member of the *Royal Academy* of *Sciences* at *Berlin*.

It was agreed to make the electrical Circuit of two Miles, in the middle of which an Observer was to take in each Hand one of the Extremities of a Wire, which was a Mile in Length. These Wires were to be so disposed, that this Observer being placed upon the Floor of the Room near the electrifying Machine, the other Observers might be able in the same View to see the Explosion of the charged Phial and the Observer holding the Wires, and might take notice of the Time lapsed between the discharging the Phial and the convulsive Motions of the Arms of the Observer in consequence thereof; inasmuch as this Time would shew the Velocity of Electricity, through a Space equal to the Length of the Wire between the coated Phial and this Observer.

The

The electrifying Machine was placed in the ſame Houſe as it was laſt Year. We then found ourſelves greatly embarraſſed by the Wire's being conducted by the Side of the Road, which we were compell'd to, on account of the Space neceſſary for the meaſuring of Sound: But ſo great a Diſtance from the Machine was not now wanted, though the Circuit through the Wire was intended to be at leaſt two Miles. We had diſcover'd by our former Experiments, that the only Caution now neceſſary was, that the Wires conducted upon dry Sticks ſhould not touch the Ground, each other, or any Non-Electric in a conſiderable Degree in any Part of their Length: if they did not touch each other, the Returns of the Wire, be they ever ſo frequent, imported little, as the Wire had been found to conduct Electricity ſo much better than the Sticks. It was therefore thought proper to place theſe Sticks in a Field fifty Yards diſtant from the Machine. The Length of this Field being eleven Chains or 726 Feet, eight Returns of the Wire from the Top to the Bottom of the Field made ſomewhat more than a Mile. and ſixteen Returns more than two Miles, the Quantity of Wire intended for the Electricity to paſs through to make the Experiment.

We had found laſt Year, * that, upon diſcharging the electriſed Phial, if two Obſervers made their Bodies Part of the Circuit, one of which graſped the leaden Coating of the Phial in one Hand, and held in his other one Extremity of the conducting Wire; and if the other Obſerver held the other Extremity of

* *See* Page 66.

of the conducting Wire in one Hand, and took in his other the ſhort iron Rod with which the Exploſion was made; upon this Exploſion, I ſay, they were both ſhocked in the ſame Inſtant, which was that of the Exploſion of the Phial. If therefore an Obſerver, making his Body Part of the Circuit, was ſhocked in the Inſtant of the Exploſion of the charged Phial in the middle of the Wire, no Doubt would remain of the Velocity of Electricity being inſtantaneous through the Length of that whole Wire. But if, on the contrary, the Time between making the Exploſion, and ſeeing the Convulſions in the Arms of the Obſerver holding the conducting Wires, was great enough to be meaſured, we then ſhould be able to aſcertain its Velocity to the Diſtance equal to half the Quantity of Wire employed only, let the Manner of the Electricity's diſcharging itſelf be what it would.

It has been a Queſtion with ſome, who have conſider'd this Subject, whether the Electricity, in compleating the Circuit from the Matter contained in the Glaſs, paſſed, either by the Wire in the Mouth to the Coating of the Glaſs, the contrary Way by the Coating to the Wire in the Mouth, or otherwiſe directed itſelf both Ways at once? That the Electricity muſt paſs off one of theſe three Ways was certain, as the Exploſion would not be complete, unleſs in the Inſtant thereof ſome Matter very non-electric communicated between the Wire in the Mouth, and the Coating of the Glaſs. Unleſs therefore the Obſerver was placed in the Centre of the conducting Wires, it might be objected, that the Experiment was not made with the Exactneſs neceſſary; becauſe any Perſon, who was of Opinion, that the Electricity

directed

directed itself from the Mouth of the Glaſs to the Coating, might object, if the Wire from the ſhort iron Rod to the Obſerver was only half the Length of that between the Obſerver and the Coating of the Glaſs, that the Electricity, in the Time found, paſſed only through the ſhort Wire; and *vice verſa*. But if, as it was here thought proper, the Obſerver was placed in the Centre of the conducting Wire, let the Direction of the Electricity be what it would, no Difference could happen in the Reſult of the Experiments, if made with the neceſſary Caution; becauſe, if the Effects in the Middle and both Ends of the Wires were inſtantaneous, the Concluſion therefrom would be very obvious.

To make the Experiment, the ſame Phial filled with Filings of Iron, and coated with Sheet-Lead, which was uſed laſt Year, was placed in the Window of the Room near the Machine, and was connected to the prime Conductor by a Piece of Wire. To the Coating of this Phial a Wire was faſtened, which, being conducted upon dry Sticks to the beforementioned Field, was carried in like manner to the Bottom, and being conducted thus from the Bottom of the Field to the Top, and from the Top to the Bottom ſeven other times, returned again into the Room, and was held in one Hand of an Obſerver near the Machine. From the other Hand of this Obſerver, another Wire of the ſame Length with the former was conducted, in the ſame manner, and returned into the Room, and was faſten'd to the iron Rod with which the Exploſion was made. The whole Length of theſe Wires, allowing ten

Yards for their Turns round the Sticks, amounted to two Miles a Quarter and six Chains, or 12276 Feet.

As the Night preceding these Experiments had been very rainy, Care was taken, by silk Lines properly disposed, that the Wires in their Passage from the Window of the House might not touch the Wood thereof; lest, from the Moisture of this Wood, the electrical Circuit might be shortened.

When all Parts of the *Apparatus* were properly disposed, several Explosions of the charged Phial were made; and it was invariably seen, that the Observer holding in each Hand one of the Extremities of these Wires was convulsed in both his Arms in the Instant of making the Explosions.

Instead of one, four Men were then placed holding each other by the Hand near the Machine, the first of which held in his right Hand one Extremity of the Wire, and the last Man the other in his left. They were all seen convulsed in the Instant of the the Explosion. Every one who felt it, complained of the Severity of the Shock.

It was then desired by one of the Gentlemen concerned, that an Explosion should be made with the Observer holding only one of the Wires. This was done accordingly; but the Observer felt nothing, the Phial discharging itself in a different manner to what it did before, on account of the Circuit's not being completed.

It was then tried, whether an Observer would be shocked upon the Discharge of the Phial, if the two Wires at their Extremities slightly touched each other, whilst an Observer at the same time held one of these about a Foot from their Ends in each of his Hands? Upon Trial he felt nothing, though the

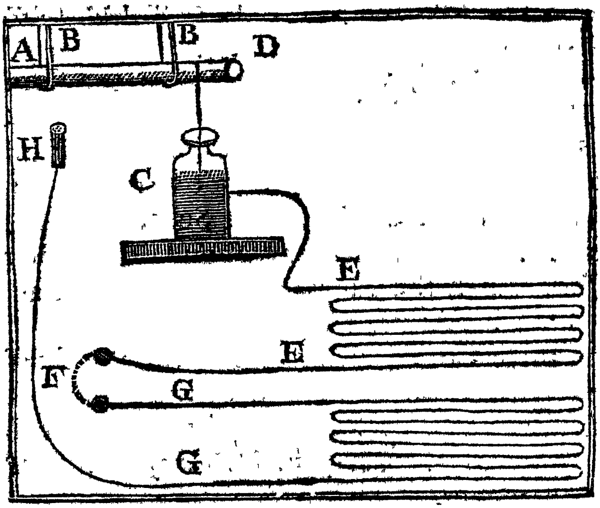
Phial

Phial exploded very quick, becauſe the iron Wire conducted the Electricity better than the Body of the Obſerver.

It was then tried, whether or no, as the Ground was wet, if the Exploſion was made with the Obſerver holding the Extremity of each Wire ſtanding upon the Ground near the Window of the Houſe, any Difference would ariſe in the Succeſs of the Experiment? No Difference was found, the Obſerver being ſhocked in the Inſtant of the Exploſion as before in both his Arms, and acroſs his Breaſt.

Upon theſe Conſiderations we were fully ſatisfied, that through the whole Length of this Wire, being as I mentioned before, twelve thouſand two hundred and ſeventy-ſix Feet, the Velocity of Electricity was inſtantaneous.

As it was found laſt Year, we obſerved again, that although the electrical Commotions were very ſevere to thoſe who held the Wires, the Report of the Exploſion at the prime Conductor was little, in compariſon of that which is heard when the Circuit is ſhort. From whence it was conjectured, that the very loud Report, in the Experiment of *Leyden* is confined to a very ſhort Circuit.



A, The prime Conductor.
B B, The ſilk Lines.
C, The coated Phial.
D, Its Hook communicating with the prime Conductor.
E E, The Wire reaching from the Coating of the Phial to the left Hand of the Obſerver, being more than a Mile in Length.
F, The Place of the Obſerver.
, A ſuppoſed Line, drawn upon the Exploſion through his Body and Arms.
G G, Another Wire, of the Length of *EE*, which goes from the right Hand of the Obſerver to *H*.
H, The ſhort iron Rod to make the Exploſion.

2. *Some*

2. *Some further Inquiries into the Nature and Properties of* Electricity; *by* William Watson *F. R. S.*

Read Jan. 21. 1747-8.

§ I. I PRESENTED to the *Royal Society October* 29. last a Paper containing some Accounts of what had been done by some Gentlemen of the *Society*, in order to examine, not only to what Distance the electrical Power was perceptible, but also to investigate, as near as might be, the respective Velocities of Electricity and Sound: Electricity indeed is the Subject of the present Paper, yet, as it relates to *Phænomena* thereof different from those mention'd in the former, I thought proper to separate them.

§ II. I took notice, in my Sequel to the Experiments relating to Electricity *, of an Observation of the ingenious Professor *Bose* of *Wittemberg*, *viz.* 'that if the electrifying Machine is placed upon 'Originally-Electrics, the Man who rubs the Globe 'with his Hands, even under these apparently favourable Circumstances, gives no Sign of being electrised when touched by an unexcited Non-electric. 'But if another Person, standing upon the Floor, 'does but touch the Globe in Motion with the End 'of one of his Fingers, or any other Non-electric, 'the Person rubbing is instantly electrised, and that 'very strongly.' This Experiment, almost a Year since, Dr. *Bevis* carried further, by placing whatever

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* Printed for *C. Davis*, *Lond.* 1746. 8*vo.* p. 32.

ever Non-electric touched the Globe as a Conductor, whether it were a Man or a Gun-barrel, upon Originally-Electrics. If then, either the Man who rubbed the Globe, or he who only held his Finger near the Equator thereof, were touched by any Person ſtanding upon the Floor, a Snapping from either of them, I ſay, was perceptible upon that Touch.

§ III. As in my Sequel I had aſſerted, and by many Experiments therein had endeavoured to evince, that, contrary to the received Opinion, the Electricity was not derived from Glaſs, the Air, or other Electrics *per ſe*, I was deſired to conſider how far this Experiment did not prove the reverſe of that Aſſertion; inaſmuch as neither the Man who rubbed the Globe, or he who touched it with his Finger, from their being here both ſupported by Originally-Electrics, could receive any Supply from the Floor; and yet both of them ſnapped upon the Touch of a Perſon not ſupported by Electrics *per ſe*. Many Experiments had proved that the Electricity was not derived from the Glaſs; and therefore it was concluded, by Dr. *Bevis*, and ſeveral others to whom this Gentleman ſhewed the Experiment, that the Electricity here was communicated to the Perſon rubbing from the Air, by means either of the ſuſpended Gun-barrel, or of the Man who touched the Globe.

§ IV. I was by no means ſatisfied with this Concluſion, as being directly contrary to numberleſs Facts. From a careful Conſideration therefore of the Experiment itſelf, from comparing its Effects with thoſe of ſeveral others, and, in general, from ſurvey-

ing all the Properties of Electricity we are hitherto acquainted with, I gave the following as my Opinion.

1. That what we call Electricity is the Effect of a very subtil and elastic Fluid, diffused throughout all Bodies in Contact with the terraqueous Globe (those Substances hitherto termed Electrics *per se* probably excepted), and every-where, in its natural State of the same Degree of Density.

2. That this Fluid manifests itself only, when Bodies capable of receiving more thereof than their natural Quantity are properly disposed for that Purpose; and that then, by certain known Operations, its Effects shew themselves by attracting and repelling light Substances, by a snapping Noise, Sparks of Fire, *&c.* directed towards other Bodies, having only their natural Quantity, or, at least, a Quantity less than those Bodies from which these Snappings, *&c.* proceed.

3. That no Snapping is observed in bringing any two Bodies near each other, in which the Electricity is of the same Density, but only in those Bodies in which the Density of this Fluid is unequal.

4. That this Snapping is greater or less, in proportion to the different Densities of the Electricity in Bodies brought near each other, and by which Snapping each of them becomes of the same Standard.

5. That Glass, and other Bodies, which we call Electrics *per se*, have the Property of taking this Fluid from one Body, and conveying it to another, and that in a Quantity sufficient to be obvious to all our Senses.

6. That, in the Experiment in Question, the Reason why no Snapping is observed by a Person upon

on the Floor touching him who rubs the Globe with his Hands ſtanding upon Wax, without at the ſame time ſome other Non-electric ſupported by Originally-Electrics, or otherwiſe being in Contact with the Globe, is owing to whatever Part of this Man's natural Quantity of Electricity, taken from himſelf by the Globe in Motion, being reſtored to him again by the Globe in its Revolutions; there not being any other Non-electric near enough to communicate the Electricity to; and that therefore, in this Situation, the Electricity of this Man ſuffers no Diminution of its Denſity.

7. That the Fact is otherwiſe, when every thing elſe being as before, either a Gun-barrel ſuſpended in ſilk Lines, or a Man ſupported by Wax, or ſuch-like, is placed near the Globe in Motion; becauſe then, whatever Part of the Electricity of the Perſon rubbing is taken from him, is communicated either to the other Man or to the Gun-barrel, theſe, from their Situation, being the firſt Non-electrics, to which the Electricity taken from the Perſon rubbing can be communicated.

8. That, under theſe Circumſtances, as much Electricity as is taken from the Perſon rubbing, is given to the other; by which means the Electricity of the firſt Man is more rare than it naturally was, and that of the laſt more denſe.

9. That the Electricity in either of theſe Perſons is in a very different State of Denſity from what it naturally was, or from that of any Perſon ſtanding upon the Earth; this laſt being in a middle State between the two other Perſons; that is, he has not his Electricity ſo rare as the Man rubbing

bing the Globe, nor so dense as that of him supported by Electrics *per se*, and touching the Equator of the Globe.

10. That therefore the same Effect, a Snapping, is observed, upon bringing any Non-electric near either of these Persons, from very different Causes: For it is apprehended, that, by bringing the Non-electric near him, whose Electricity is more rare, this Snapping restores to him what he had lost; and, that, by bringing it near him, whose Electricity is more dense, it takes of his Surcharge, by which means their original Quantity is restored to each.

§ V. This Solution of this *Phænomenon*, without allowing any Part of the Electricity of either of these two Persons to be furnished by the circumambient Air, was satisfactory, not only to the Gentleman who proposed it, but to many of the *Royal Society*, excellent Judges of this Matter, to whom I shewed the Experiment: And this the more so, as it is to be observed, that if, under the before-mention'd Circumstances, the Person rubbing the Globe was touched by him who held his Finger to the Globe, the Snapping was much greater than if either of them touched a Person standing upon the Floor; as the Density of the Electricity between these two Persons was so much more different than that of either of them to him on the Floor: Whereas did their Electricity proceed from the Air, from their being both electrised they ought not to snap at all from their touching each other; or, admitting they did touch each other, they both of them, upon a Supposition that they did receive their Electricity alike from the Air, should manifest the Accumulation thereof, and snap

upon the Touch of a Man ſtanding upon the Floor, the contrary of which invariably happens.

§ VI. At this time I am the more particular concerning the Solution of this ſingular Appearance, as Mr. *Collinſon*, a worthy Member of this *Society*, has received a Paper concerning Electricity from an ingenious Gentleman, Mr. *Franklin*, a Friend of his in *Penſylvania*. This Paper, dated *June* 1. 1747. I very lately peruſed, by Favour of our moſt worthy *Preſident*. Among other curious Remarks there is a like Solution of this Fact; for though this Gentleman's Experiment was made with a Tube inſtead of a Globe, the Difference is no-ways material. As this Experiment was made, and the Solution thereof given upon the other Side of the *Atlantic Ocean* before this Gentleman could poſſibly be acquainted with our having obſerved the ſame Fact here, and as he ſeems very converſant in this Part of Natural Philoſophy, I take the Liberty of laying before you his own Words.

'1. A Perſon ſtanding on Wax, and rubbing a 'Tube, and another Perſon on Wax drawing the 'Fire; they will both of them, provided they do 'not ſtand ſo as to touch one another, appear to 'be electriſed to a Perſon ſtanding on the Floor; 'that is, he will perceive a Spark on approaching 'each of them with his Knuckle.

'2. But if the Perſons on Wax touch one another during the exciting of the Tube, neither of 'them will appear to be electriſed.

'3. If they touch one another after the exciting 'the Tube and drawing the Fire as aforeſaid, there 'will be a ſtronger Spark between them, than was 'between either of them and the Perſon on the 'Floor.

'4.

‘ 4. After ſuch a ſtrong Spark neither of them ‘ diſcover any Electricity.

‘ Theſe Appearances we attempt to account for ‘ thus:

‘ We ſuppoſe, as aforeſaid, that electrical Fire is ‘ a common Element, of which every one of ‘ theſe three Perſons has his equal Share before ‘ any Operation is begun with the Tube. *A*, ‘ who ſtands upon Wax, and rubs the Tube, ‘ collects the electrical Fire from himſelf into the ‘ Glaſs; and his Communication with the com- ‘ mon Stock being cut off by the Wax, his Body ‘ is not again immediately ſupplied. *B*, who ‘ ſtands upon Wax likewiſe, paſſing his Knuckle ‘ along near the Tube, receives the Fire which ‘ was collected by the Glaſs from *A*; and his ‘ Communication with the common Stock being ‘ cut off, he retains the additional Quantity re- ‘ ceived. To *C* ſtanding on the Floor, both ap- ‘ pear to be electriſed: For he, having only the ‘ middle Quantity of electrical Fire, receives a ‘ Spark upon approaching *B*, who has an over ‘ Quantity, but gives one to *A*, who has an un- ‘ der Quantity. If *A* and *B* approach to touch ‘ each other, the Spark is ſtronger; becauſe the ‘ Difference between them is greater. After ſuch ‘ Touch, there is no Spark between either of them ‘ and *C*, becauſe the electrical Fire in all is re- ‘ duced to the original Equality. If they touch ‘ while electriſing, the Equality is never deſtroyed, ‘ the Fire only circulating. Hence have ariſen ‘ ſome new Terms among us. We ſay, *B* (and ‘ Bodies alike circumſtanced) is electriſed poſi-

 ‘ tively;

'tively; *A*, negatively; or, rather, *B* is electrised
'*plus*, *A*, *minus*. And we daily in our Experi-
'ments electrise *plus* or *minus*, as we think pro-
'per. To electrise *plus* or *minus*, no more needs
'be known than this; that the Parts of the Tube
'or Sphere that are rubbed, do in the Instant of
'the Friction attract the electrical Fire, and
'therefore take it from the Thing rubbing. The
'same Parts immediately, as the Friction upon
'them ceases, are disposed to give the Fire, they
'have received, to any body that has less. Thus
'you may circulate it, as Mr. *Watson* has shewn *;
'you may also accumulate or substract it upon or
'from any Body, as you connect that Body with the
'Rubber, or with the Receiver, the Communi-
'cation with the common Stock being cut off.'

The Solution of this Gentleman, in relation to this *Phænomenon*, so exactly corresponds with that which I offer'd very early last Spring, that I could not help communicating it.

§ VII. In Sect. 51. and 62. of my Sequel to the electrical Experiments, which I presented to the *Royal Society*, last Year, from not having consider'd this Experiment in a statical View, and from not then imagining the Velocity of Electricity so great as we since have found it, I concluded, that the Snapping observed, if a Person standing upon the Floor touched the Man standing upon Wax, who turned the Wheel of the electrifying Machine placed likewise upon Wax, to be owing to the Inversion of the usual Course of the Electricity; as that Snapping was only constant, when the

* *See my* Sequel, *p.* 64.

the Gun-barrel ſuſpended in ſilk Lines was touched by Non-electrics. As from divers Experiments I had found that Electricity was not furniſhed by dry Air, by many more that it could not come down clean ſilk Lines; and as, from his Snapping, the Man upon the Wax argued the Preſence of Electricity, I conceived that this could happen no other Way, than that the rubbing of the Globe by a Cuſhion or the Hand of a Man, gave it a Fitneſs to take off the Electricity, furniſhed by the ſuſpended Gun-barrel from the Non-electric upon the Floor, and lodge it upon the Machine, and upon the Man who turned the Wheel thereof. But the Experiment of circulating the electrical Fire *, where the Bruſh of blue Flame from a blunt Wire properly diſpoſed, can always be ſeen to paſs diverging into the Machine, though not ſo, when brought near the Gun-barrel under the moſt favourable Circumſtances; as well as the Experiment before-mention'd brought to ſhew that the Electricity came from the Air, have induced me to change my Opinion; and inſtead of the Courſe of the Electricity being inverted, the *Phænomena* aroſe, as far as I am capable of judging, from the Man who turned the Wheel of the electrifying Machine having leſs than his original Quantity of Electricity, and the Gun-barrel from having more: To theſe add, that the Perſon, who touched theſe while ſtanding upon the Floor, had a Quantity different from each of theſe, that is, his natural Quantity.

I beg

* See *Sequel* § 65.

§ VIII. I beg Leave to correct alſo what I mention'd in my *Sequel*, p. 69. in relation to my ſuggeſting, that, in the Exploſion of the charged Phial through the Body of a Man, or other Non-electrics, as much Electricity as was taken from his Body, was immediately replaced by the Floor of the Room upon which he ſtood: I having ſince found, that the charged Phial would explode with equal Violence, if the Hook of the Wire, which is uſually run through the Cork of the Phial, was bent in ſuch a manner as to come near the Coating of the Phial, without any other Non-electric being near, from which ſuch Quantity could be ſupplied.

§ IX. I take notice of theſe, inaſmuch as, notwithſtanding the very great Progreſs that has been made in our Improvements in this Part of Natural Philoſophy within theſe few Years, Poſterity will regard us only as in our Noviciate; and therefore it behoves us, as often as we can be juſtified therein by Experiment, to correct any Concluſions we may have drawn, if others yet more probable preſent themſelves.

§ X. I laid down and conſider'd largely in my Sequel *, that the Stroke from the Phial, in the Experiment of *Leyden*, was not in Proportion to the Quantity of Matter contained in the Glaſs, but was increaſed by the Quantity of Matter in the Glaſs, and the Number of Points of non-electrical Contact on the Outſide of the Glaſs. This Fact I have purſued further, and increaſed thereby the electrical Exploſion

* See *p.* 11, 17, 19, *&c.*

ſion to an aſtoniſhing Degree. To this End I procured three cylindrical Phials blown very thin, about ſeventeen Inches in Height and four in Diameter: After theſe were coated within an Inch of their Necks with Sheet-Lead, I put into each fifty Pounds of leaden Shot. I choſe this Form for the Glaſſes, that the Matter therein contained might be expoſed under as large a Surface, as could conveniently be obtained. Theſe Glaſſes were placed near each other in a convenient Part of my Room, and did communicate with each other by means of a ſmall iron Rod lying upon all their Mouths, and touching Pieces of ſtrong Wire ſtuck into the Shot contained in them: By this Management one of theſe could not be electriſed without communicating with the reſt. The leaden Coatings of theſe Glaſſes were alſo connected together by ſmall Wires, all which center'd in one tail Wire; ſo that, when the Matter contained in theſe three Glaſſes was replete with Electricity, which was done by a Wire from the Gun-barrel faſtened to the iron Rod lying upon their Mouths, the whole Quantity of Electricity here accumulated might be diſcharged at once by touching the Gun-barrel with an iron Rod faſtened to the tail Wire. When the Glaſſes are ſufficiently electriſed, if the Room is dark, you will ſee Bruſhes of blue Flame from ſeveral Parts of the conducting Wire; and theſe indicate the proper Time of making the Exploſion. Theſe Glaſſes, from the Thinneſs of their Sides, and from the Weight of their leaden Shot, are very liable to burſt; and if one of them happens to have the leaſt Crack in any Part of its Surface, which is under the Lead, none of them can be electriſed; all the Electricity

paſſing

paſſing off by that Crack. The electrical Exploſion from two or three of theſe Glaſſes is not double or treble to that from one of them; but the Exploſion from three is much louder than that from two, that from two much louder than that from one.

§ XI. The Experiment juſt mentioned induced me to imagine, that the Exploſion from theſe Phials was owing to the great Quantity of non-electric Matter contained in them: And whilſt I was conſidering of ſome certain Method of aſſuring myſelf whether the Fact were ſo, Dr. *Bevis* informed me, that he had found the electrical Exploſion to be as great, as when he had accumulated the Electricity in a half Pint Phial of Water, by the following Method. He covered a thin Plate of Glaſs, of about a Foot ſquare on both Sides, with Leaf-Silver; this he made to adhere to the Glaſs with very thin Paſte. A Margin of an Inch was left on both Sides; otherwiſe, upon electrifying this Plate, the Electricity would be prevented from being accumulated upon one of its Surfaces, by being propagated from the Silver on one Side to that of the other. When the glaſs Plate was thus prepar'd, if it was placed upon a Table in ſuch a Manner, that, when fully electriſed by a Wire or ſuch-like from the prime Conductor, a Perſon touched the under Surface with a Finger of one of his Hands, and brought one of the Fingers of his other near the upper Surface thereof, or near the prime Conductor, he was ſhocked in both his Arms and acroſs his Breaſt. The ſame Effect happen'd, if, when this Plate was electrified in the before-mention'd Manner, a Perſon holding it in his Hand by the Margin, and without touching the Sil-

ver,

Silver presented it, even some time after it had been taken from the prime Conductor, to another Person who touched the under Surface with his Finger, and held it there till he touched the upper Surface with a Finger of his other Hand.

§ XII. This Experiment was sufficiently convincing, that the Greatness of the electrical Explosion, in my former Trials, was not owing solely to the great Quantity of non-electric Matter contained in the Glasses; as the Explosion from the glass Plate silvered was occasioned by about six Grains of Silver, upon which the Electricity was accumulated; more especially as this Explosion was equal, if not superior, to that from half a Pint of Water contained in a thin Glass as usual, under the most favourable Circumstances.

§ XIII. As each of the Surfaces of the glass Plate just mentioned measured sixty-four square Inches, I was desirous of pursuing this Inquiry further; and accordingly procured a cylindrical glass Jar blown very thin, of sixteen Inches in Height, and eighteen Inches in Circumference. This I caused to be covered both within and without with Leaf-Silver, to within an Inch of its Top. This Glass with its Margin made very clean (upon which the Success of the Experiment considerably depends) was fully electrised by the means of a Piece of Chain, let down to the Bottom of the Jar, by a Wire from the prime Conductor; and the Explosion made by its being placed upon a Plate of Metal, to which was fasten'd a Wire connected to an iron Rod, and this Rod was

brought near ſome gilded Leather lying upon the prime Conductor. This Exploſion was equal to that from the three Glaſſes before-mention'd, containing a hundred and fifty Pounds of leaden Shot; though here the Weight of the Silver lining the internal Surface of the Glaſs, upon which the Electricity was accumulated, did not exceed thirty Grains. So much of the internal Surface of this Jar, as was cover'd with Silver, amounted, as the Surfaces of Cylinders are as their Length multiplied by their Periphery, and allowing thirty ſix ſquare Inches for the Bottom, to three hundred and ſix ſquare Inches. If this Exploſion was made in a dark Room, the Corruſcations within the Jar, at the Inſtant of the Exploſion, were extremely brilliant.

When this Jar is fully electriſed, if, inſtead of making it explode, you only bring the ſhort iron Rod, with which the Exploſion is uſually made, near a Piece of gilded Leather lying upon the prime Conductor, though not near enough to make the Glaſs explode at once, you hear the Electricity, accumulated within the Jar, eſcape with a Noiſe very like that of a ſmall heated iron Bar quenching in Water.

§ XIV. The great Exploſion from the Jar before-mention'd, when ſo little non-electric Matter was included therein, has cauſed me to be of Opinion, that the Effect of what we call the Experiment of *Leyden* is greatly increaſed, if not principally owing, not ſo much to the Quantity of non-electrical Matter contained in the Glaſs, as to the Number of Points

of

of non-electrical Contact * within the Glass, and the Density † of the Matter constituting those Points, provided this Matter be in its own Nature a ready Conductor of Electricity. For this Reason it is presumed, that so much of the Lead contained in the Shot in the before-mention'd Experiment, only concurred to make the electrical Explosion, as touched the internal Surface of

* Bodies having the Power of readily conducting Electricity seems to depend very little upon their specific Gravity simply considered: Metals, for Instance, and Water, are in a great Degree Non-electrics, and consequently conduct Electricity the best of any Substances, that have yet fallen under our Notice; whereas the *Calces* of Metals, though very dense Bodies, and very greatly more so than Water, prevent in a great Degree the quick Propagation of the electrical Power. So that a Phial coated within and without with Ceruse, *i. e.* the *Calx* of Lead, and electrised, did not, upon the Application as usual of one Hand to the external Surface thereof, and touching the prime Conductor with the other, occasion any Shock, or make any Explosion more than the simple Stroke from the prime Conductor. The same Observation holds good with regard to red Lead, Litharge, and lunar Caustic or the *Calx* of Silver, none of which snap, when electrised. For the same Reason, Filings of Iron, which are rusty, *i. e.* have their Surfaces converted into a *Calx*, are much less proper to be put in Glasses to make the Experiment of *Leyden*, than those that are not; inasmuch as these last cause a much louder Explosion than the first. The making use of rusty Filings of Iron was the Occasion of my mentioning in my *Sequel*, § XVI. that the Stroke from these was less than that from Water; the contrary of which I afterwards found true, when Filings of Iron not rusty were substituted.

† I heretofore, *p.* 11, &c. of my *Sequel*, took notice, how much the Effect of this Experiment depended upon the Quantity of non-electric Contact upon the Outside of the Glass.

of the Glaſs: As a great Part of this Surface was without Contact, occaſioned by ſuch of the Shot as preſented themſelves thereto, touching, from their ſpherical Figure, only in one Point, there conſequently remained without Contact comparatively great Spaces between each Shot. This Defect was obviated by the univerſal Contact of the Silver, and thereby was occaſion'd the greater Exploſion.

§ XV. The following Experiment has ſome Relation to the preceding. If a Phial of warm Water, without being coated with Sheet-Lead, or other non-electrical Matter, is electriſed by connecting it to the prime Conductor; and a Ring of ſmall Wire, in lieu of the uſual Coating, is put round this Phial, the Wire being continued of a ſufficient Length to touch the prime Conductor; upon diſcharging the Phial, you have a ſlight Exploſion, and a Flaſh of Fire ſeems at that Inſtant to fill the Glaſs. But if this Experiment is made in a very dark Room, and with great Attention, this Flaſh in the Phial will not then ſeem to proceed from the whole Quantity of Water contained therein; but, as far as the Suddenneſs of the Exploſion will permit the Eye to follow it, will be ſeen to occupy only the internal Surface of the Phial.

§ XVI. I order'd another glaſs Jar as large as poſſible to be blown, ſo that the Glaſs thereof might be very thin; and after many Attempts of the Glaſs-makers I procured one, the Height of which was twenty two Inches, the Periphery forty-one. This was covered within and without, leaving a Margin

of

of an Inch at Top, with Leaf-Brafs. As much of the internal Surface as was covered amounted to 1129 fquare Inches. But the Difficulty I met with in procuring this Glafs was fufficiently recompenfed by the great Increafe of the Explofion therefrom, when fully electrifed, and difcharged in the fame manner as the glafs Jar before-mention'd. The Report was vaftly louder; all the attendant *Phænomena* greatly exceeded any thing of this kind I was before acquainted with. As the Quantity of Metal within this Jar did not exceed two Drams, this Experiment gives further Weight to my Opinion before-mention'd § XIV. in relation to the manner of increafing the Effects of the Experiment of *Leyden*; and from what the *Phænomena* of that furprifing Experiment principally proceed; *viz.* not from the Volume of the prime Conductor, nor from the Quantity of non electrical Matter contained in the Glafs, but from the Number of Points of non-electrical Contact both within and withoutfide of the Glafs, and from the * Denfity of the Matter conftituting thofe Points.

§ XVII. It muft be obferved, that, *cæteris paribus*, the electrical Explofion is greater from hot Water included in Glaffes than from cold; and from thefe glafs Jars warmed than when they are cold.

§ XVII.

* Though the Denfity of the Matter conftituting thefe Points proceeds from their Number in a mathematical Senfe, yet in a popular one I take the Liberty to diftinguifh them.

§ XVIII. The Explosions from the large Glasses just mention'd fully electrised, as well as from small ones under the same Circumstances, will not be considerable, unless the Circuit, frequently mention'd in my Writings upon this Subject, be completed; that is, unless some Matter, non-electric in a considerable Degree, and in Contact with the Coatings of the Phials, is brought into Contact, or nearly so, with such Non-electrics as communicate with the Matter contained in the Phials themselves. When indeed the Circuit can be completed, the Explosion from the large Glasses is prodigious; the whole Quantity of Electricity therein accumulated, or nearly so, being discharged in an Instant. But the Fact is otherwise, if the Circuit is not completed, and the iron Rod in the Mouth of one of these Phials is touched by a Non-electric (the Hand of a Man, for Instance) not in Contact with the tail Wire: For then there will be no Explosion, no Shock; but the Person, approaching his Finger near the iron Rod, will see a Succession of small Sparks, more intensely red than that large one seen, when the Phials explode at once; and the Person making the Experiment, will feel a very pungent Pain, but confined to that Finger which touches the iron Rod. This Succession of Sparks continues, until the Electricity accumulated in the Phials is nearly exhausted. So that the Explosion from any given Quantity of Electricity, accumulated as before-mention'd, is greater or less in proportion to the Time expended in making that Explosion: In like manner as a given Quan-

tity of grained Gunpowder rammed hard in a Piſtol, is almoſt inſtantaneouſly fired, and that with a great Report; when the ſame Quantity of Gunpowder rubbed fine, and rammed hard, takes a conſiderable Time in burning as a Squib, and makes no Exploſion.

§ XIX. The Cauſes why the charged Phial will not explode quick, without the Electricity therein deſcribing a Circuit through Subſtances non electric in a great Degree, may be very difficult to be aſſigned. It is ſufficient for us in the preſent Inquiry to be aſſured of its being a certain, an invariable Law: And in order to prove, that the Electricity, upon the Exploſion, paſſes with its whole Force through the Circuit of Non-electrics, contrary to what has been ſuggeſted, I made the following Experiment.

§ XX. I procured two ſmall ſquare iron Bars, of about fourteen Inches long: An Inch at each End of theſe I cauſed to be bent at right Angles. Theſe iron Bars were ſupported in ſuch manner (by Subſtances whether originally electric, or not, was noways material) that each of their Ends came within about two Tenths of an Inch of ſome warm Spirit of Wine, or Eſſence of Lemons in four Spoons placed upon a Table. I then ſuſpended a common coated Phial filled with Filings of Iron to the Gun-barrel, the tail Wire of which reached to a Table at a few Feet Diſtance, and was placed under a braſs Weight which ſupported the Handle of the firſt of the Spoons: Over this Spoon, at the Diſtance juſt mention'd, I placed one of the ſquare

iron

iron Bars, and at its other End was placed another Spoon: This second Spoon touched the Handle of the third, which was placed under one End of the other square Bar, whose other End came near to the Spirit in the fourth Spoon, the Handle of which lay upon a Weight; and under this was placed a Wire connected to the short iron Rod, with which the Explosion was made, when the coated Phial was charged. When the Phial was well charged, if the Spirit of Wine sent forth Vapours, and the square iron Bars were at a proper Distance from it; upon making the Explosion at the Gun-barrel the Electricity snapped between the Spirit and the iron Bars, and the Spirit was set on Fire at the same Instant in all the Spoons. It sometimes happened, that some of them only were fired. If the iron Bars were too near the Spirit, it was not fired, though the Circuit was completed; because then no electrical Flame snapped between the Rods and Spirit; that Effect happening only, when the Parts of the Non-electrics describing the Circuit are not in immediate Contact; on the other hand, if the Space left between the Bars and Spirit was too great, the Circuit could not be completed, and there would be no Explosion.

§ XXI. This Experiment will seem more surprising in the following manner. When the *Apparatus* is disposed of as before, the tail Wire from the coated Phial, before it reaches to the Table, is fasten'd to an iron Rod standing in a Pail of Water: Another iron Rod is likewise placed in the same Pail of Water, and a Wire from this last reaches under the Weight, which supports the first of the

before-

before-mention'd Spoons. From beneath the Weight which supports the Handle of the fourth Spoon, a Wire reaches to an iron Rod standing in a second Pail of Water, in which is placed also another iron Rod, to which is fastened another Wire connected with the short iron Rod, which is employed to make the Explosion. When, with this Disposition of the *Apparatus*, the charged Phial is caused to explode, the Spirit or Essence of Lemons in some or all of the Spoons is set on Fire; to accomplish which, the Electricity must necessarily pass through one of the Pails of Water, and possibly through both. But here it must be understood, that the Pails of Water stand upon a dry wooden Floor; for if they stand upon one that is wet, or upon the Ground, the Circuit will be, for Reasons frequently mention'd in the Course of these Inquiries, completed between the two Pails, where the non-electric Matter is continuous, and be prevented from passing by the Spoons where it is not so; and this will defeat the Success of the Experiment. The Number of Spoons in the Manner before-mention'd, and their Distance from each other, may be varied as far as is thought necessary. The Circuit may likewise be directed through any Number of Men, provided that each of them holds in one of his Hands a Spoonful of warm Spirit, and brings one of the Fingers of his other Hand at the proper Distance to the Spirit held in the Hand of the Person next him: By these means the Explosion of the charged Phial will set on Fire the Spirit in several of the Spoons at

the ſame time, provided the Perſons employ'd hold their Hands ſufficiently ſteady.

§ XXII. This Experiment exhibits new and unexpected *Phænomena:* In all the Experiments to kindle inflammable Subſtances by Electricity hitherto attempted both here and abroad, either the Spirit or the Non-electric, wherewith it was intended to be ſet on Fire, were placed upon Originally-Electrics. But here, on the contrary, although both one and the other are placed upon Non-electrics, we ſee the ſame Effect produced. Nor is the electrical Power leſſened, by exciting ſeveral different Quantities of Flame; in doing which, it paſſes ſo quick as to prevent the Poſſibility, in ſeveral Spoonfuls of Spirit, fired by the ſame Operation, of determining which of them was on Fire firſt: And though we know from its Effects, that the Electricity goes through the whole Circuit of Non-electrics with its whole Vigour, its Progreſs is ſo quick as not to affect, by by attracting or otherwiſe, light Subſtances diſpoſed very near the Non-electrics, through which it muſt neceſſarily paſs.

§ XXIII. I would here recommend to thoſe Gentlemen of the *Royal Society*, who laſt Summer meaſured the reſpective Velocities of Electricity and Sound, a Proceſs of this ſort to be executed at a proper Time; whereby they would be able to a very great Nicety to aſcertain the abſolute Velocity of Electricity. For it may be contrived, that a Man may be placed in the ſame Room with the electrifying Machine, taking hold of a Wire in each of his Hands:

These

These Wires may be ſo managed, that by means of the electrical Circuit, the Man holding them may be made ſenſible of the electrical Commotion, even under the Eye of an Obſerver at the Machine; though before the Electricity can arrive at the Perſon holding the Wires, it will be obliged to paſs through whatever large Space ſhall be thought convenient for the Obſervation. The Time then ſpent tween the Exploſion of the charged Phial, and the Perſon holding the Wires feeling the electrical Commotion, will give the abſolute Velocity of Electricity to great Exactneſs *.

XXIV. As my Inquiries upon the Subject of Electricity have always tended as much as poſſible to the Analyſis thereof, I have often obſerved, that if, when the electrifying Machine ſtands upon the Floor, the Globes thereof are rubbed with their Cuſhions, or with Hands cover'd with Originally-Electrics of a ſufficient Thickneſs, and perfectly dry, no Electricity will be perceptible upon the Touch of a Gun-barrel ſuſpended in ſilk Lines, and touching the Globe in Motion, or upon the Touch of any other Subſtances ſupported by Electrics *per ſe*; or, in other Words, there will be no Accumulation of Electricity. The only Originally-Electrics fit for this Experiment (as all unctuous Subſtances, as Wax, Reſin, and ſuch-like, though Electrics *per ſe*, by ſticking to the Outſide of the Glaſs render it unfit to excite Elec-

tricity

* This has been ſince put in Execution. *See above* p. 88.

tricity from other Bodies) are to be obtained from the Animal Kingdom: And of these only such as do not partake, from their Manufacture or otherwise, of any non-electric Substances. Those of this sort, which I have tried, and always with the same Success, when perfectly dry, have been Silk (woven or not), Velvet, Hair-Cloth, Woollen-Cloth, and the dry Skins of Rabbits dressed in their Fur; and the Event has been the same, whether these Substances have been rubbed under a greater or a less Degree of Friction: And scarce any Electricity has been perceptible, when those Parts of these Substances, which immediately are in Contact with the Globes, have been rubbed over with dry Chalk, a non-electric Substance. But the Success is different, when these originally-electric Substances have lain in damp Places, or have been held over the Steam of warm Water; because then the Water imbibed by these Substances serves as a Canal of Communication to the Electricity between the Hands or Cushions and the Globes in the same manner, as the Air, replete with Vapours in damp Weather, prevents the Accumulation of Electricity in any considerable Degree, by conducting it as fast as excited to the nearest Non-electrics. On the contrary, most Substances of the Vegetable Kingdom, whose Form makes them fit for this Treatment, though made as dry as possible, furnish Electricity, though in different Quantities. I have tried Hemp, Linen-Cloth of various Kinds, Paper both of Linen and Hemp, Cotton in the Wool, Fustian, Cotton-Velvet,

and

and many others of this Claſs. I have covered at one time the Cuſhion, with which I rubbed a Globe, with eight Lamina of Sheet-Lead, and have excited Electricity from that Metal: And however improper a Deal-Board may ſeem for the Purpoſe of rubbing a Globe, I have more than once accumulated Electricity from that, though its Subſtance has the Appearance of being much leſs fit than every one of the Originally-Electrics I mention'd before.

§ XXV. To the Doctrine here laid down it may be objected, that Leather is an animal Subſtance, which, though perfectly dry, excites Electricity the ſtrongeſt of all the Subſtances hitherto diſcover'd; that dry Leather ought to be conſider'd as an Originally-Electric; and therefore, according to the Rule before-mention'd, ſhould not furniſh, from rubbing the Globe therewith, any Electricity at all. To this I anſwer, that though the dry Skins of Animals are Electrics *per ſe*, dry Leather is far from being ſo; and this is owing to the vaſt Quantities of reſtringent vegetable Subſtances imbibed by the Skins throughout their whole Contexture in the Operation of Tanning in ſome Species of Leather, and of ſaline Subſtances, ſuch as Alum, in others; both which Subſtances are non-electric, and of theſe Leather very conſiderably partakes: For by theſe the Hides and Skins of Animals (and any Muſcle of their Bodies is liable to the ſame Treatment), which otherwiſe are as putreſcent as any Part of their Bodies ſoever, are made to laſt through many Ages, and be ſubſervient to

many

many valuable Purposes of Life. The same Conclusion must be drawn concerning Hats, which, tho' made of the Hair of Animals, furnish Electricity, though but in a small Degree: And this is occasion'd by the mucilaginous and gummy Substances made use of by the Hatmakers, to give their Manufacture a suitable Stiffness.

§ XXVI. From what I have advanced § XI. XII. XIII. XIV. XV. XVII. it may possibly be conjectur'd, that the electrical *Effluvia* occupy only the Surfaces of Bodies electrised; as we there found, that a very small Quantity of Matter, distributed under a very large Surface, would occasion a greater Accumulation of Electricity, than a very much more considerable Quantity of Matter under a less. But that the Electricity occupies the whole Masses of Bodies electrised, and passes through their constituent Parts, is clearly demonstrated by the following Experiments.

§ XXVII. When I first engaged in these Inquiries, to assure myself of this Fact, I enveloped an iron Rod about three Feet in Length with a Mixture of Wax and Resin, leaving free from this Mixture only one Inch at each End. This Iron was warmed, when thus fitted, that the whole of its Surface, where it was intended, might be covered. This Rod, when electrised at one of its Ends, snapped as strongly at the other, as though it was without the Wax and Resin. This could not have happened from the Electricity's passing along the Surface of the iron Rod, because there it was prevented

by

by the Originally-Electrics, and consequently must of Necessity pass through it.

§ XXVIII. A Phial of Water, in the Experiment of *Leyden* can be electrised, and may be caused to explode, though the Wire, touching the Water in the Phial in making that Experiment, be run through a wax Stopple, exactly fitted to the Mouth of the Phial.

§ XXIX. I caused a glass Tube, open at each End, and about two Feet and a half long, to be capped with Brass cemented to the Ends of the Tube. In the Centre of each of these Caps was fastened a slender brass Rod; and these were disposed so in the Tube as to come within half an Inch of each other. When the Tube was properly suspended in silk Lines with one of its Extremities near a glass Globe in Motion, the brass Work at both Ends snapped equally strong. As the Electricity could not pass along the Surface of this Tube warmed and wiped clean, this Effect could not have happened, unless the Electricity pervaded the Substance of the brass Caps. Upon touching the Brass at the End of the Tube most remote from the electrifying Machine, the Snaps from one of the brass Rods within the Tube to the other were seen to correspond with the Snaps without. More Experiments of this kind might be added, but these, I presume, are sufficient to shew, that the Electricity occupies the whole Masses of non-electric Bodies electrised. That the Electricity passes through Originally-Electrics to a certain Thickness I took notice of in a Paper I did myself the Honour to communicate in *February* 1745.

§ XXX.

§ XXX. I ſhall forbear at preſent to lay before you a Series of Experiments *in vacuo*; from the Compariſon of which, with the Experiments in open Air it appears, that our Atmoſphere, when dry, is the Agent, whereby, with the Aſſiſtance of other Electrics *per ſe*, we are enabled to accumulate Electricity in and upon Non electrics; that is, to communicate to them a greater Quantity of Electricity than they naturally have: From hence alſo we ſhall ſee, that, upon the Removal of the Air, the Electricity pervades the *Vacuum* to a conſiderable Diſtance, and manifeſts its Effects upon any Non-electrics, which terminate that *Vacuum*: And by theſe Means that originally-electric Bodies, even in their moſt perfect State, put on the Appearance of Non electrics, by becoming the Conductors of Electricity. But theſe Matters may poſſibly be the Subject of a future Communication.

IV. *A Letter from Mr.* Francis Drake, *Surgeon, F. R. S. to* Martin Folkes *Esq*; President *of the* Royal Society, *&c. concerning the Bones of a* Fœtus *being discharged thro' an Ulcer near the Navel.*

Worthy Sir, *York, June* 22. 1747.

Read Jan. 28. 1747. HAVING a Call from hence into *Lincolnshire* lately, to see a Patient, the Apothecary who attended on him informed me, amongst other things, of an extraordinary Case, which had happened in that Neighbourhood, a very few Years ago. I have since been informed, on Inquiry, that it has not as yet been represented to the *Royal Society*; and therefore I hope you will do me the Honour to lay this Account of the Case before them.

Jane, the Wife of *James Burman*, Labourer, at *Scawby* near *Brigg* in *Lincolnshire*, was about 29 Years of Age when she married. About two Years after, when she had had a Child at full Time, she conceived again, and went regularly on for four Months. She then got a Fall, and about three Weeks after felt a Load in her Belly, which continued, on the right Side of the same, for between two and three Years. The Woman then grew very big with another Child, which pressed so much upon the Lump as to give her great Uneasiness. However, she went on to her Time with her double Burden; and, three Years and a Quarter after the accidental Fall, she

was deliver'd of a live Child at full Growth: From which Time ſhe grew worſe and worſe, with violent Pain about the Navel, and an inflamed Tumour appeared near that Part. Upon Application to a neighbouring Surgeon, Fomentations were uſed, which produced a Suppuration at a ſmall Breach near the Navel. The Surgeon did not know what to make of this Swelling, and therefore did not venture to enlarge the Orifice; but it continued diſcharging a fetid purulent Matter for three or four Months longer.

About a Year, or more, after her laſt Delivery, the Woman was ſuddenly ſeized in the Night-time, and a hardiſh Maſs of Fleſh, ſeemingly about eight Inches long, was diſcharged thro' the old Opening in her Belly. The Lump was rather thicker than an ordinary Man's Wriſt, and which being opened, contained all the Bones of a *Fœtus*, of about four Months Growth.

At this time the Woman was much emaciated, occaſioned by the large Diſcharge of *Pus* from the Wound; and, what was much more extraordinary, whatever ſhe eat or drank came half-digeſted thro' the Opening. White Bread, or better Diet, came thro' in that Manner; but coarſe Rye-Bread, or ſuch-like, was not digeſted at all. For which Reaſon, the poor Woman muſt have inevitably periſhed, had ſhe not been ſupported by a charitable Gentleman's Family in the Village, with Diet fit for her miſerable Circumſtances.

She continued to diſcharge her Excrement in this Manner for ſix Months, and then that Symptom left her; after which the Ulcer was kept open other ſix Months,

Months, when it dried up of itſelf naturally, with a very firm but ſmall Cicatrix.

I had the Curioſity to ſee this Woman, and Mr. *Charleſworth*, Surgeon and Apothecary at *Brigg*, ſent for her. She appeared hale, ſtrong, and in full Health. I had the above Account of her Caſe from her own Mouth, atteſted by the Surgeon who attended her. I ſaw the Bones of the *Fœtus* in Mr. *Charleſworth*'s Poſſeſſion, perfectly white, and, I believe, not one wanting. The Woman further told me, that, nine Months after the Wound was healed, ſhe was deliver'd of another live Child, at full Time, but with great-Difficulty. The whole Time that the Bones of the *Fœtus* may be ſuppoſed to have lain in the Woman's Belly was about four Years and a-half.

Thus, Sir, I have drawn up the Account as well as I can, but very inaccurately. I have purpoſely omitted Terms of Art, in order to make myſelf better underſtood by thoſe who are not Surgeons or Anatomiſts. There are ſeveral Particulars in the Account, which I cannot reconcile to any natural Laws that I am acquainted with: However, as the Truth of the Whole is inconteſtable, it ſhews moſt evidently what wonderful Things Nature can do, with proper Aſſiſtance.

I am, Sir, with my beſt Reſpects to the Gentlemen of the *Society*,

Your moſt obliged, and
moſt obedient humble Servant,

Francis Drake.

V. *An Account of the* Giants Causeway *in* Ireland, *in a Letter to the* President *from the Rev.* Richard Pococke, *LL.D. Archdeacon of* Dublin, *and F.R.S.*

Dublin, 5 *Jan.* 1747-8.

Read Jan. 28. 1747.

IN my last Passage over to this Kingdom, I saw that very remarkable Curiosity, commonly called the Giants Causeway:§ The Sea-Cliffs are very high thereabouts, and what is called the Causeway is a low Head, extending from the Foot of the Cliffs into the Sea like a Mole. This Head does not appear at first so grand as it is represented in the Views engraved of it; but when one comes to walk upon it, and consider it more attentively, it appears to be a stupendous Production of Nature. The Head ends in two Points: I measured the more Western to the Distance of 360 Feet from the Cliff, and it appeared to me to extend about 60 Feet further; but this Part I could not measure, by reason that the Sea was then high; and I was told, that at low Tides it might be seen about 60 Feet yet further upon a Descent losing itself in the Sea. I also measur'd the more Eastern Point 540 Feet from the Cliff, and saw about as much more of it as of the other, when it winds about to the Eastward, and is also lost in the Water. One may walk upon this Head on the Tops of the Pillars to the Edge of the Water. These Pillars are of all angular Shapes from three Sides to eight. The Eastern Point, towards that End where it joins the Rocks, terminates it self for some Way in a perpendicular

§ This Causeway is before taken notice of in N°. 199, 212, 235, and 241 of these *Transactions*.

perpendicular Cliff, formed by the upright Sides of the Pillars, ſome of which I meaſured, and found to be 33 Feet and 4 Inches in Height. They ſay there are in all 74 different Sorts of Figures among them. Each Pillar conſiſts of ſeveral Joints or Stones lying one upon another, from 6 Inches to about a Foot in Thickneſs: Some of theſe Joints are in the middle ſo convex, as for thoſe Prominences to be nearly Quarters of Spheres, round each of which is a Ledge, upon which rhe Stones above them have reſted, every Stone being concave on the under Side, and fitting in the exacteſt manner upon that which lies next below it. The Pillars are from one to two Feet in Diameter, and conſiſt moſt commonly of about 40 Joints, moſt of which ſeparate very eaſily, tho' ſome others, which are more ſtrongly indented into each other, cohere ſtrongly enough to bear the being taken away in Pairs.

But the Cauſeway is not I think the moſt ſingular Part of this extraordinary Curioſity; the Appearance of the Cliffs themſelves being yet to me more ſurpriſing; theſe and their ſeveral *Strata* I examined from the Rocks on the other Side of a little Bay, about half a Mile to the Eaſt of the Cauſeway. I thence obſerved, that there runs all the Way a *Stratum* from the Bottom of black Stone, to the Height, as well as I could conjecture, of about 60 Feet, divided perpendicularly at unequal Diſtanees by Stripes of a reddiſh Stone, looking like Cement, and about 4 or 5 Inches in Thickneſs. Upon this there is another *Stratum* of the ſame black Stone divided from it by a *Stratum* 5 Inches thick of the red. Over this another *Stratum* of Stone ten Feet thick divided in the ſame manner; then a *Stratum* of the red Stone twenty Feet deep; and above that a *Stratum* of

of upright Pillars. Above theſe Pillars lies another *Stratum* of black Stone 20 Feet high; and above this is again another *Stratum* of upright Pillars riſing in ſome Places to the Top of the Cliffs, in others not ſo high, and in others again above it, where they are called the Chimneys.

This Face of the Cliffs reaches for two computed Miles Eaſt from the Cauſeway, that is about 3 meaſured *Engliſh* Miles, to the Houſe of Mr. *John Stewart* two Miles Weſt of *Balintoy*. The upper Pillars ſeem to end over the Cauſeway, and, if I miſtake not, become ſhorter and ſhorter as one goes from it, lying between two Binds of Stone like Seams of Coal, and like thoſe little Pillars found in *Derbyſhire* §.

Theſe Binds probably meet together all round, and incloſe this extraordinary Work of Nature; and if ſo, the Pillars muſt be very ſhort towards the Extremities.

I was led to this Conjecture by the following Obſervations: The lower *Stratum* of Pillars is that which goes by a Deſcent into the Sea, and which makes what is called the Giants Cauſeway; and where this Deſcent approaches the Sea, it ſeems probable that the Pillars become ſhorter and ſhorter, ſo as to end not much further off. Now the upper Bind of this *Stratum* may have been of ſo ſoft a Nature, as by degrees, in Proceſs of Time, to have been waſhed away by the Sea. And in the Cliff over the Cauſeway I ſaw ſeveral Pillars lying along in a rude manner almoſt horizontally, which ſeemed to me to be ſome of the Pillars of the upper *Stratum* fallen down by the giving way of the Bind which

§ The Doctor here refers to a Paper of Mr. *Emanuel Da Coſta*, communicated *May* 14. 1747. but not yet printed.

which was under them, and over the lower ones that compose the Causeway. And here most probably the upper Pillars ended, as they are seen no farther in the Cliff. I saw the Tops of Pillars even with the Shore, both on the the East and West Sides of the Causeway, and some much lower than the Causeway itself; and it is probable that these are much shorter than those of the Causeway, which I measured above thirty Feet higher than the Tops of them.

When I was upon the Causeway, I saw in the Cliff, to the South-east, what they call the Organs, about a Quarter of a Mile off, and a third Part of the Way up the Cliff. They appeared small, and somewhat like a black *Stalactites*: They were not commonly known to be such Pillars as the others; but they are so, and belong to the lower *Stratum*. When with great Difficulty I climbed up the steep Hill to them, I found they were hexagonal, and larger Pillars than most of the others, being about 2 Feet in Diameter; and I measured 5 Sides of one of them, which were of 13, 15, 12, 21, and 16 Inches respectively. The Joints I could come at were about 9 Inches thick, and each Pillar, as well as I could count, consisted of between 40 or 50 of them: These Joynts are almost flat and plain, the Convexities on their upper Faces being so small as to be scarce discernible. I enquired whether any of these Pillars were found in the Quarries within Land, and the People there told me they were not; but since I left the Place, I have been assured by others, that there are some found two or three Miles from the Shore. I am, with the greatest Regard, Sir,

Your most obedient humble Servant,

Richard Pococke.

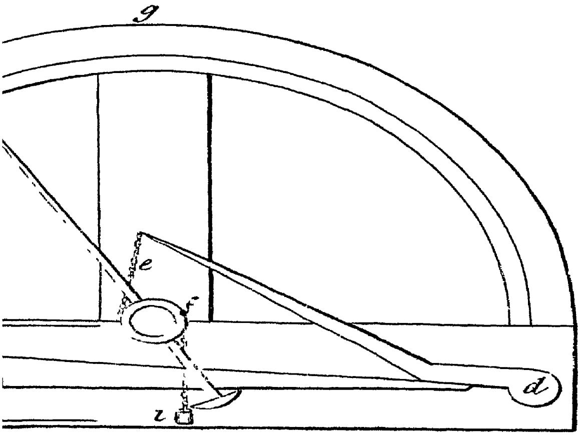
VI.

VI. *A Letter from* Maurice Johnſon *Eſq*; Preſident *of the* Gentlemens Society *at* Spalding, *to* James Jurin *M. D. Fellow of the* Royal College *of* Phyſicians, London, *and F. R. S. concerning a* Metalline Thermometer, *in the* Muſeum *of that* Society.

S I R, *Spalding, Jan.* 16. 1747-8.

Read Jan. 28. 1747.

AS I know it muſt give you Pleaſure, and, being by you (as I deſire it may be at their next Meeting) communicated to the *Royal Society*, may be of ſome Credit to the Memory of the Inventor the late Mr. *Samuel Frotheringham*, a Grazier at *Holbeach* in *Elloe Holland*, *Lincolnſhire*, and of ſome Profit to the Maker, give me Leave to acquaint you, that he (Mr. *John Ingram*, of this Place, Watch-maker and White-Smith, whoſe Father, originally a Black-Smith at *Cowbitt*, and Inventor of the Machine for cutting Watch-Wheels, was alſo a moſt accurate Artificer) having made, and, at my Inſtance ſome time ſince, fixed up in our *Muſæum*, a Metal Thermometer, which we, on Experience and Obſervation, found to anſwer and go truly, I propoſed to the Company, at our laſt Meeting in *December*, that our *Society* ſhould purchaſe it him, I ſend you, Sir, his Deſcription thereof, as enter'd from his Mouth in our Minutes, which I truſt may be agreeable to you, and the worthy Members of that Illuſtrious Body, for which we here have the higheſt Honour: And though Mr. *Beridge*



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Beridge (ſome time a Watch-maker at *Boſton*) under the ſame Inventor's Direction, made and carried up to Town a Machine ſomewhat of this ſort, which ſeveral of your Members may have ſeen, yet I truſt this Account may not be unacceptable. I am

Your moſt affectionate humble Servant,

M. Johnſon.

A Deſcription of the Metal Thermometer *in the* Muſeum *of the* Gentlemens Society *at* Spalding *in* Lincolnſhire.

IT is compoſed of an upright Staff or Bar (*a*) of the beſt Iron, four Feet long, and an Inch and a Quarter broad, having a poliſh'd braſs Bar of the ſame Length and Width ſcrew'd to it before it, with four Steel Screws, and being alſo capp'd (*b*) with Steel, and thereon a Lever (*c*) moving upon a Stud of Steel, which communicates with another leſs Lever (*d*) (alſo upon a Stud) having a Chain (*e*) at the End of it, which laps round an *Axis* (*f*), whereto the Index is fixed, which ſhews the Degrees marked on a ſemicircular Arch (*g*): Under the Steel Screw-Heads there are ſmall Slits in the Braſs Bar (except the lowermoſt which is fixed) which admit of its expanding, whereby it protrudes and operates on the firſt-mention'd Lever, which being raiſed moves the leſs Lever, and thereby draws the Chain which turns the Axis affixed to the Index, which ſhews the

Degree of Warmth of the Weather marked on the femicircular Arch. At (*h*) is a Screw thro' two Studs, to draw the great Lever backwards and forwards, as Occafion may be; (*i*) is a Counter-balance to the fmall Lever to draw the Hand back when the Brafs Bar fhrinks. *See the* Figure *prefixed in the* TAB.

In the Beginning of the Year 1735. I invented, and caufed to be conftructed, a Thermometer on the fame Principles as this: I found that a Rod of Brafs 3 Feet long was fenfibly affected by the Changes of Heat of the Weather, having one expofed in my Garden during the hard Froft of the Winter 1739 and 40. And my Inftrument was very fenfible with either a Brafs Rod or an Iron Rod, when the Bottom of it was placed in a Sand-Heat for chemical Ufes; but I fhall refer the Reader to the Appendix to the preceding N°. p. 672. & *feq.* wherein I have given, a full Defcription of my Invention, and the Reafons why I did not publifh it before; tho' I have fhewn the Inftrument to Scores of People ever fince *May* 1735. and fent a Defcription and Draught of it to M. *Buffon*, Superintendant of the *Royal Phyfick Garden* at *Paris* in the Year 1744. in order for his laying it before the *Royal Academy* of *Sciences* at *Paris*, from which I had fome time before received a *Diploma* upon having the Honour of being appointed one of their *Correfponding Members*.

C. Mortimer.

ERRATA.

Page 1. *l.* 9. *for* Febr. 14. *read* Jan. 7 and 14. *P.* 27. in the Column expreffing the Nutation,

1729. Sept. 8. *for* — 6.9 *read* — 6.4
1730. Sept. 8. *for* — 3.4 *read* — 3.9

Numb. 486.

PHILOSOPHICAL TRANSACTIONS.

For the Months of *February* and *March*, 1748.

IV.

X.

I. *An Abstract of the remarkable Case and Cure of a Woman, from whom a* Fœtus *was extracted, that had been lodged thirteen Years in one of the* Fallopian Tubes; *sent from* Riga *by Dr.* James Mounsey, *Physician to the Army of the* Czarina, *together with the Bones of the said* Fœtus, *as a Present to the* Royal Society *of* London, *and delivered accordingly in his Name, by* Henry Baker, *F. R. S.*

The Original read at a Meeting of the Royal Society, Feb. 4. 1747-8.

THE Particulars of this extraordinary Case are set forth at large in * Dr. *Mounsey's* Paper, the Substance whereof is as follows:

A Soldier's Wife, of *Abo* in *Finland*, of a middling Stature, and who had been the Mother of two Children, being pregnant a third time in the Year 1730. was afflicted with violent Pains and Twistings in the Bowels, swooning Fits, Vomitings, and great Disorders in her Back and lower Belly. These Symptoms, and Complaints of several other kinds, continued to make her uneasy, till she found her Burden

* It is likewise printed in the *Swedish* Language, in the Acts of the *Royal Academy* at *Stockholm*.

Burden increaſe, which fell from Side to Side as ſhe changed the Poſture of her Body, and could be preſſed by her Hand from one Place to another, but rather choſe to remain on the right Side.

After Quickening her Health became better, ſhe grew bigger belly'd than ordinary, and was ſuppoſed to carry Twins. About the time her Delivery was expected, ſhe was taken ill with violent Pains by Fits acroſs her lower Belly; but had none in her Back, nor any Forcings downwards; and next Day theſe Pains went off, which made her ſuppoſe ſhe had miſreckon'd. But after this her Breaſts ſwell'd, and gave Milk in Plenty; and her Menſes came on, attended with more violent Pains than ſhe had had before; and ſuch large Diſcharges of Blood from time to time, that ſhe could neither ſpeak nor move; and even after the Floodings were ſtopp'd, her Blood and Strength ſeem'd quite exhauſted: She often fainted away, and was ſometimes thought to be dead.

She continued very ſickly for ten Years afterwards; during which Time her Burden was moveable, and fell from Side to Side. But in the Month of *September* 1741. ſhe felt a Pain beneath the Navel, with a Swelling and Redneſs, which in about three Weeks appear'd like a ſmall Boil. This ſhe pierced with an Awl, and a yellow-colour'd Water ran from it without any Smell, and continued ſo to do for near three Weeks more, when it diſcharged a purulent ſtinking Matter.

In the Month of *June* two ſmall Bones came out, which were given to the Surgeon that viſited her; who only applied a Piece of Plaiſter, perſuading her that a Cure was impoſſible. Other ſmall Bones

work'd

work'd themſelves out afterwards, till *October* 1742, when the Head-Quarters of the *Ruſſian* Army being at *Abo*, this unhappy Woman applied to Dr. *Mounſey*, who, after a careful Examination, undertook to deliver her. And accordingly, deſiring the Aſſiſtance of Mr. *Geitle*, Surgeon to the Regiment of *Abo*, a grooved Probe was thruſt into the *Fiſtula*, and an Inciſion made with a Biſtory, upwards and obliquely, from the *Linea alba* into the Cavity of the *Abdomen*; but ſhe being unruly, and the Operation not going on to the Doctor's Liking, he proceeded no farther till the next Day; only ſome looſe Bones were extracted, and the Wound dreſſed with Tents and Compreſſes, to hold in the *Omentum*, and keep the Wound open.

At the next Operation the Inciſion was carried downwards, and then another Inciſion continued from the firſt was conducted upwards, and ſlanting at a ſmall Diſtance from the firſt; taking care to keep as near as poſſible the Direction where the Adheſion of this Body to the *Peritonæum* appeared to be, and avoiding to make the external Wound larger than abſolutely needful, leſt the *Omentum* and Guts ſhould fall out; and particularly leſt the Suppuration ſhould exceed the Strength of Nature, which was here already nearly exhauſted.

A large Opening was now made, but the *Fætus* being cloſely enveloped by its containing Sack, the Doctor durſt not venture to draw it out by Force, for fear ſome of the naked Bones might lacerate the internal Parts: Wherefore, dilating this Sack with the Points of a Pair of Probe-Sciſſars, directed by the Fingers of the left Hand, he pierced

and cut in Pieces the Skull, which afterwards he extracted piece-meal.

The Matter that first issued out had a very nauseous Smell, and consisted of Membranes, Fat, and corrupted Flesh. On opening the *Cranium*, the *Cerebrum* appeared of its natural Colour. The Operation having been long, and the Woman fainting away, the Wound was dressed, without attempting to extract more at that time. In the Evening she was taken with Vomitings; but by proper internal Medicines, and Flannel Stoups wrung out of hot Wine, applied over the whole Belly, and often renew'd, she found Ease, and grew better. —— The Loss of Blood, during the whole Operation, was inconsiderable,

Next Day the Bones of the Trunk, and most of the other large ones, with their Ligaments and rotten Flesh, were taken out. The Matter discharged for several Days was of a dark-brown Colour, occasion'd by Blood issuing from the dilated Pores of the internal Surface of the Sack, which render'd the Matter at first of a deep-red Colour; but that changed daily and gradually, till at last it became white. The Doctor imagines this Discharge to resemble the *Lochia* after Child-birth; for after the *Fœtus* was extracted, the Woman's Breasts swell'd, and gave Milk in Plenty for two Months, in Quantity, Colour, and Consistence, as if after a Delivery at the proper Time.

The Doctor examined this Sack very diligently with his Fingers, whilst thin, and not contracted, but formed into Wrinkles, through which he felt the *Rectum*, the *Vesica urinaria*, and, as he thought, the

the *Fundus Uteri.* Many ſmall Bones lay in the Folds; but, as ſhe complained not of Pain, they were left till the Suppuration began, excepting thoſe the Sack in contracting itſelf threw out.

Beſides Fomentations, Balſamics, proper Bandages, *&c.* vulnerary detergent Injections were found very uſeful, thrown in in large Quantities, both to waſh out the putrid Fleſh, and bring away the Scales of Bones which were ſtill concealed in the Folds of this Sack.

The Sack contracted itſelf daily, grew ſmooth and white within, and narrower as it approached the *Uterus*, which gave Reaſon to believe it one of the *Fallopian* Tubes.

The Wound was cured in about ſix Weeks, and the Woman deliver'd from a long State of Miſery grew fat and luſty, and now enjoys good Health.

Comparing theſe Circumſtances together, it ſeems reaſonable to believe this Fruit never was in the Cavity of the Womb, but that the impregnated *Ovum* was ſtopt in its Paſſage through one of the *Fallopian* Tubes, where it grew, and was detained ſo many Years; and that the Inflammation, which happened below the Navel, was not owing to the Rottenneſs of the *Fœtus*, or to its bare Bones ſeeking a Diſcharge, but rather to ſome accidental Friction of the containing Sack againſt the *Peritonæum*, thereby producing Adheſion, Obſtruction, Inflammation, *&c.* The *Fœtus*, before this Accident, muſt have remained all theſe Years intire, and without perfect Corruption: For it took no leſs Time, after its Communication with the common Air, before it ſhewed

shewed Marks of Putrefaction, than a fresh Subject, kept in the same Degree of Heat, would have done.

The Doctor's Observations of the Bones are: That they have a full Proportion to those of a Child at nine Months, and that the Fibres are more compact, and their Articulations stronger. The Sockets for the Teeth are six on each Side of the Jaws; the *Dentes Incisores* of the upper Jaw are high and large; the *Molares* have almost all begun to ossify in their *Alveoli*; at least the Crown, which is the cortical Part, is form'd, and they are fill'd internally with a cretaceous Substance. In new-born Children those Parts are seldom found so far advanc'd, which gives Reason to believe this Child did not die within the ordinary Time of Pregnancy, and that the different Accidents, before-mention'd to have happen'd, were chiefly owing to the preternatural Situation of the *Fœtus*.

Some Places in the Skull appear to have been carious, and corroded by some sharp Humours; and Nature, supplying its ossifying Juices, has repaired these Places, and render'd them more solid and whiter than the rest, but very uneven and scabrous, from the different Times and Directions of the bony Sproutings. There are likewise *Exostoses* on the Ends of the Thigh-Bone, and some other Bones.

'Tis very difficult to determine about what Time the Growth of these Productions began or ended. Supposing it from the Time of the Disorders that happened in the first Months of Pregnancy; would not such a Disease have caused Death to the *Fœtus*, before it had come thus to a full Growth? If it was the Consequence of the violent Accidents which

happen'd about the Time of the natural Birth, the Child then must have continued alive some considerable Time afterwards, during which these bony Excrescences were formed; there being a perfect Ossification, as performed by the Laws of Circulation, and not by any vegetative or petrifying Power, as in inanimate Bodies.

Two or three of the lateral Processes of the Spine were what first passed thro' the little Ulcer; the rest of the Bones (except a few that were lost in cleaning) were presented by the Doctor to the Museum of the *Royal Society*. They retain a very strong and singular Smell, though they were immediately cleansed from the rotten Flesh, and well washed.

The Woman came by Sea to *Stockholm* above a Year after this Cure, and was presented to the *Academy* in good Health; and the Doctor believes she is still alive and well.

II. *The Motion of* Projectiles *near the Earth's Surface consider'd, independent of the Properties of the Conic Sections; in a Letter to* Martin Folkes *Esquire, Pr. R. S. by Mr.* Tho. Simpson *F. R. S.*.

Read Feb. 4. 1747.

AFTER so much as has been already said upon the Motion of Projectiles *in vacuo*, it may seem needless to attempt any thing further on that Head; nevertheless, as a thorough Knowledge in the Art of Gunnery is become more than

than ever necessary, and as Gentlemen employ'd in the Practice of that Art are (I am sensible) too often deterr'd from applying themselves to the Theory, by the Difficulties they imagine they shall meet with in the Conic Sections, you will, I hope, pardon the Liberty I have taken, in troubling you with my Thoughts on a Subject, in which little or nothing new is to be expected besides the Method.

When I first drew up this Paper (which was about two Years ago) I did intend, had Health permitted me to make the proper Experiments, to have also attempted something with respect to the Resistance of the Atmosphere, whereof the Effects are indeed too considerable to be intirely disregarded: But if the Amplitude of the Projection, answering to one given Elevation, be first determined by Experiment (which our Method supposes) the Amplitudes in all other Cases, where the Elevations and Velocities do not very much differ from the first, may be determined, by the Proportions here laid down, to a sufficient Degree of Exactness: Because, in all such Cases, the Effects of the Resistance will be nearly as the Amplitudes themselves; and were they accurately so, the Proportions of the Amplitudes, at different Elevations, would be exactly the same as *in vacuo*; which Proportions I now proceed to determine.

PROBLEM I.

Let two Balls be projected with the same Celerity, at different, but given Elevations, 'tis proposed to determine the Ratio of the Times of their Flight,

Flight, of their greatest Altitudes, and of their horizontal Amplitudes.

Let Pq (*Fig.* 1.) represent the Plane of the Horizon, PEQ and peq the Paths of the Projectiles, described in the Flight; moreover let QPT and qpt be the given Angles of Elevation, and let PQ and pq be bisected in H and h; drawing HE, QT, he and qt, all perpendicular to Pq; and making the Sine of $QPT=S$, its Co-sine $=C$, the Sine $qpt=s$, its Co-sine $=c$, and Radius $=r$.

Therefore, since the Distances descended by heavy Bodies (whether from a Point at Rest, or from the right Lines in which they *would* move, if not acted upon by Gravity) are known to be as the Squares of the Times, QT will be to qt, as the Square of the Time of describing PEQ (or of that wherein the Ball would move uniformly over the Space PT with its first Velocity at P) is to the Square of the Time of describing peq (or of that wherein the other Ball would move uniformly thro' the Length pt). But the Celerities at P and p being equal, by Hypothesis, the Times in which the said Lines PT and pt would be uniformly described, are manifestly, as the Lines themselves: Whence the Squares of those Lines must, also, be as the Squares of the Times, and, consequently, as the Distances descended: that is, $Pt^2:pt^2::TQ:tq$.

Now, by Plane Trigonometry $TQ=\frac{S\times PT}{r}$ and $tq=\frac{s\times pt}{r}$; therefore $PT^2:pt^2$ $(::\frac{S\times PT}{r}:\frac{s\times pt}{r})::S\times PT:s\times pt$; whence, by dividing the Antecedents

by PT, and the Consequents by pt, we have PT : pt :: S : s; from which it appears, that the Times of Flight are directly as the Sines of Elevation.

Again, the Times of describing EQ and eq (which are the Halves of the Wholes) being also to one another as S : s, and the Distances EH, eh descended in them, as the Squares of the Times, it likewise follows, that $S^2 : s^2 :: EH : eh$; or that the greatest Altitudes are as the Squares of the Sines of Elevation.

Moreover, because (by Trigonometry) $PT = \frac{r \times PQ}{C}$ and $pt = \frac{r \times pq}{c}$, and it has been already proved, that, $S : s :: PT : pt$, it follows, that $S : s :: \frac{r \times PQ}{C} : \frac{r \times pq}{c}$; whence, by multiplying the Antecedents by $\frac{2C}{r}$ and the Consequents by $\frac{2c}{r}$, it will be $\frac{2SC}{r} : \frac{2cs}{r}$ $(:: 2PQ : 2pq) :: PQ : pq$. But $\frac{2SC}{r}$ is known to be the Sine of double the Angle whose Sine is S, and Co-sine C, *&c.* Therefore the horizontal Amplitudes are to one another, as the Sines of the double Elevations.

Corol. 1.

Hence it follows, that the greatest Amplitude possible will be, when the Elevation is half a Right Angle, or 45 Degrees (because the Sine of 90° is the greatest of all others).

Corol.

Corol. 2.

Therefore, if the greateſt Amplitude be given (from Experiment) the Amplitude anſwering to any propoſed Elevation, above, or below, 45 Degrees, may from hence be found: For it will be as the Radius, to the Sine of double the given Elevation, ſo is the greateſt, to the required, Amplitude.

Corol. 3.

Hence, alſo, the Altitude of the Projection may be known; for QT, when the Angle QPT is half a Right Angle, will be $= PQ$; and therefore HE $(\frac{1}{4}TQ) = \frac{1}{4}PQ$; alſo, in this Caſe, $S^2 = \frac{1}{2}r^2$; whence our Proportion $S^2 : s^2 :: HE : he$ will here become $\frac{1}{2}r^2 : s^2 :: \frac{1}{4}PQ : he$; from whence it appears, that, as the Square of the Radius is to the Square of the Sine of any given Elevation, ſo is half the greateſt horizontal Amplitude, to the Altitude of the Projection. Hence it alſo follows, that the Height to which the Ball would aſcend, if projected directly upwards, is juſt half the greateſt Amplitude.

Corol. 4.

Therefore, ſince it is well known, that a Body *in vacuo* aſcends and deſcends with the ſame Velocity; and that the Diſtances deſcended are as the Squares of the Velocities; it follows, that the Amplitudes, at the ſame Elevation, with different Velocities, will alſo be to one another as the Squares of the Velocities; becauſe they are as the greateſt Amplitudes, with the ſame Velocities (by *Corol.* 2.)

T 2 and

and these are as the Distances perpendicularly descended (*by the precedent*). Whence, *universally*, if both the Elevations and the Velocities differ, the Amplitudes will be to each other in a *Ratio* compounded of the *Ratio's* of the Sines of double the Angles of Elevation, and of the duplicate *Ratio's* of the Velocities, or impelling Forces.

Problem II.

The Angle of Elevation, and the greatest horizontal Amplitude, being given, to find at what Distance the Piece ought to be planted, to hit an Object, whose Distance, above or below the Plane of the Horizon, is also given.

Let AB (*Fig.* 2 and 3.) be the Plane of the Horizon, BC the perpendicular Height or Depression of the Object, and AB the required Distance: Also let BC be produced to meet the Line of Direction AD in D, and let P be the Place where the Path of the Projectile would meet the Horizon; moreover, let PQ be perpendicular to AP, and CN parallel to AD. Then, by the preceding Problem, it will be as Radius : the Sine of $2BAD$ :: the given (or greatest) Amplitude : AP; which therefore, is known.

Moreover, the Areas of similar Triangles being as the Squares of their homologous Sides, we have $AP \times PQ : AB \times BD :: AQ^2 : AD^2$. But $AQ^2 : AD^2 :: AB \times BD :: QP : DC$ (from Principles already explained) therefore, by Equality, $AP \times PQ : AB \times BD :: QP : DC$; and consequently $AP : AB :: BD : CD$; but (because of the parallel Lines CN and AD) $BD : CD :: AB : AN$; whence, again

again by Equality, $AP : AB :: AB : AN$; therefore, by Division, $AP : BP :: AB : BN$; and, consequently $AP \times BN = BP \times AB$.

Let AP be now bisected in O; then $BP \times AB$ being $= AO^2 - OB^2$ (in the first Case) and $= OB^2 - AO^2$ (in the second Case), we shall therefore have $OB^2 = AO^2 \mp AP \times BN = AO \times \overline{AO \mp 2BN}$: whence the Distance AB is likewise known. *Q.E.I.*

Corollary.

Hence, if the Elevation, and the greatest Amplitude, together with the Distance AB of the Object be given, the Height or Depression of the Ball in the Perpendicular BCD will be known: For it is proved, that $AP : BP :: BA : BN$; whence BN is known: But, as the Radius to the Tangent of BNC (BAD): so is BN to BC.

Problem III.

The greatest horizontal Amplitudes of the Piece, together with the Distance and Height (or Depression) of the Object being given, to find the Direction or Angle of Elevation.

Let BC (*Fig.* 4 and 5.) be the perpendicular Height or Depression of the Object, AB its given horizontal Distance, and AH the required Direction; Also let PQ (*Fig.* 6.) be the greatest Amplitude (answering to 45° of Elevation); draw AC, in which produced (if need be) take $AG = PQ$; make MGO perpendicular to AG, meeting AB produced (if need be) in O; and from the Centre O, with the Interval OA,

let

let a Circle be described, interfecting AG, produced in E, and the Line of Direction AD in H; join E, H, and let HI, AN and QR, be perpendicular to AE, AO, and PQ refpectively, and let BC, produced, meet AH in D.

It will appear, from what has been faid above, that $AD^2 : PR^2 :: DC : RQ$; therefore PR^2 being $= 2PQ^2 = 2AG^2 = \frac{1}{2}AE^2$, and $RQ = PQ = \frac{1}{2}AE$ (by Conftruction), we have $AD^2 : \frac{1}{2}AE :: DC : \frac{1}{2}AE$, and therefore $AD^2 = AE \times DC$.

Now, the Triangles ADC, AEH, being equiangular (becaufe $ADC = DAN = AEH$, and DAC common to both) we likewife have $AD : DC :: AE : EH$, and confequently $AE \times DC = AD \times EH = AD^2$ (*per* above); whence $EH = AD$. Therefore, as the Triangles ADB and EHI are equiangular, they are equal in all refpects; and fo $HI = AB$: Whence follows this eafy

Conftruction.

Having defcribed the Circle AEF, as above directed, and drawn MG perpendicular to AE, take Gn equal to AB, and thro' n, parallel to AE, draw Hh, cutting the Circle in H and h; join A, H, and A, h; then either of the Directions AH or Ah, will anfwer the Conditions of the Problem. From this Conftruction we have the following Calculation; *viz.*

As AB is to BC, fo is AG to OG; which added to, or fubtracted from, Gn (AB) gives On: Then, it will be, as $AG : On ::$ the Co-fine of OAG : Cofine of HOn ($= HAh$) the Difference of the two required

required Elevations; whence the Elevations themselves are known. *Q. E. I.*

Corol. 1.

Hence, if the Elevation of the Piece, with the Distance and the Height (or Depression) of the Object be given, the greatest horizontal Amplitude may be found: For it will be $AB : BC$: : Radius : Tang. of BAC; whence CAD is also known.

Then, $S. CAD : S. ACD$ (AHE) : : AD (HE) : AE.

And, $S. ADC$: Radius : : $AB : AD$.

Therefore, by compounding these Proportions, we have $S. CAD \times S. ADC$: Radius $\times S. ACD$: : $AB : AE$; which is equal to twice the required Amplitude, by Construction.

Corol. 2.

Moreover, if the Elevation, and the greatest horizontal Amplitude be given, the Amplitude of the Projection on any ascending or descending Plane AE, whose Inclination FAE is also given, may from hence be derived. For, $S. AHE$ (ACD) : $S : EAH$ (CAD) : : AE ($2PQ$) : EH (AD) and $S. ACD$: $S. ADC$: : $AD : AC$; whence, by compounding the two Proportions, *Sq*. $S. ACD : S. CAD \times S. ADC$: : $2PQ : AC$; from which AC is known.

Corol. 3.

Since it appears, that the Triangles ADB and EHI are equal and alike in all respects, and, therefore, the horizontal Distance AB, *universally*, equal to the Perpendicular HI, it is manifest, that, when

HI

HI is the greateſt poſſible, *AB* will alſo be the greateſt poſſible; in which Circumſtance *AC* (if the Angle *FAE* be given) will likewiſe be the greateſt poſſible: And this, it is evident, muſt be, when *HI* coincides with *MG*, or when the Angles *HEA* and *HAE* are equal (as in *Fig.* 7 and 8); at which time the Point *D* coincides with *H*; becauſe *AD* and *EH* are always equal to each other. Therefore, ſince, in this Caſe, *HAE* (*HEA*) is $=$ *NAH*, it follows, that the Amplitude, on any inclined Plane *AE*, will be the greateſt poſſible, when the Line of Direction *AH* biſects the Angle made by the Plane and Zenith.

Corol. 4.

Hence the greateſt Amplitude on any inclined Plane may alſo be known; for the right-angled Triangles *AOG* and *HOB*, having $AO = HO$ and the Angle *O* common, are equal in all reſpects; and therefore, as Tang. of *AHG* (*BAH* the Angle of Elevation): Tang. of *CHG* (*CAB* the Plane's Inclination) :: $AG : GC$; whence $AC = AG \mp GC$ is alſo known.

Corol. 5.

Hence, alſo, if the greateſt Amplitude on an inclin'd Plane be given, the greateſt horizontal Amplitude may be determined: For, Radius : *S. BAC* :: $AC : BC = CG =$ the Difference of the given, and the required, Amplitudes.

Corol. 6.

But if, inſtead of the Plane's Inclination, the perpendicular Height, or Depreſſion, of the Object be given; then, *AC* ($AG \mp BC$) being to *BC*, as Radius

to

to the Sine of BAC, and Radius : Cotang. BAC :: $BC : AB$; the greateſt Diſtance AB, at which the Ball can poſſibly hit the Object, will from hence be given: which Diſtance (becauſe $AC = AG \mp BC$, and $AB^2 = \overline{AC + CB} \times \overline{AC - BC}$) will alſo be expreſſed by $\sqrt{AG \times \overline{AG \mp 2BC}}$. Hence the greateſt horizontal Amplitude of a Ball, projected from a given Height above the Plane of the Horizon is known: For ST (*Fig* 8.) may here be ſuppoſed to repreſent the Plane of the Horizon, and SA the given Height; and then SC, being equal to AB, is given from above $= \sqrt{AG \times \overline{AG + 2BC}}$.

Corol. 7.

But, if the horizontal Diſtance AB be given, and it be required to find the greateſt Height the Ball can poſſibly reach in the Perpendicular BCD; we ſhall have HG (AB) : AG :: Radius : Tang. of the Elevation (BAH or AHG); and Radius : Tang. BAC ($2BAH \backsim 90°$) :: $AB : BC$; which therefore is known. But (becauſe $AC \pm BC = AG$, and $\overline{AC + CB} \times \overline{AC - CB} = AB^2$) the ſame will alſo be truly exhibited by $\frac{AG^2 \backsim AB^2}{2AG}$.

Corol. 8.

Laſtly, let the Height, or Depreſſion, of the Object be given, together with its Diſtance AB, to determine the Direction, and the leaſt *Impetus* poſſible, to hit the Object: Then $AB : BC$ :: Radius : Tang. BAC; whence the Elevation BAH is known: And as Radius : Tang. AHG (BAH) :: MG (AB) : AG; whence the *Impetus* is alſo known.

III. *The Case of* Henry Axford, *who recover'd the Use of his Tongue, after having been four Years dumb, by means of a frightful* Dream; *communicated by the Rev. Mr. Archdeacon* Squire, *F. R. S.*

Read Feb. 4. 1747-8.

HENRY *Axford*, Son of *Henry Axford*, of the *Devizes* in *Wiltshire*, an Attorney, when a Child was subject to Convulsion-Fits, which followed him pretty frequently till he was about 25 Years of Age. After this, his Health became extremely good. At about 28 Years old, going with some Ladies to see *Longleat* in *Wiltshire*, the Seat of Lord Viscount *Weymouth*, he perceived a Hoarseness coming upon him, which was soon after attended with all the Symptoms of a common Cold, till, in about six Days after his first Seizure, he became quite speechless, not only losing the articulate Use of his Tongue, but being scarcely able to make the least Noise with it. His Cold quickly went off in the usual manner, and he grew perfectly well, as well in Health as ever he had been in his Life; but he still continued absolutely speechless. He had Advice from all the neighbouring Physicians, but to no purpose; for nothing they did for him could restore him to the former Use of his Tongue.

He continued in this dumb Way about four Years; till one Day in the Month of *July*, in the Year 1741, being at *Stoke* in the above-mention'd County, he got very much in Liquor, so much, that, upon his

his Return home at Night to the *Devizes*, he fell from his Horſe 3 or 4 times, and was at laſt taken up by a Neighbour, and put to Bed in a Houſe upon the Road. He ſoon fell aſleep; when, as he tells the Story himſelf, dreaming that he was fallen into a Furnace of boiling Wort, it put him into ſo great an Agony of Fright, that, ſtruggling with all his Might to call out for Help, he actually did call out aloud, and recovered the Uſe of his Tongue from that Moment as effectually as ever he had it in his Life, without the leaſt Hoarſeneſs remaining, or Alteration in the old Sound of his Voice, as near as can be diſcerned. He was not uſed to drink hard; he is ſtill alive, continues in good Health, and has the Uſe of his Tongue as perfectly as ever he had in the former Part of his Life.

IV. *Extract of a Letter from Mr.* William Arderon, *F. R. S. to Mr.* Henry Baker, *F. R. S. concerning the* Hearing *of* Fiſh.

S I R,

Read Feb. 11, 1747-8.

AS it was at your Deſire that I ſet myſelf to make Experiments and Obſervations on the Nature and Properties of Fiſh, and to diſcover, if poſſible, whether they are ſenſible of Noiſes, and of the Motions of Bodies, by Hearing, in the Manner of Land Animals; or whether, being deſtitute of that Senſe, the Want of it is ſupplied by the Quickneſs of their Sight and Feeling; I am going to lay before you the Method I made

-ufe of to come at fome kind of Certainty in this doubtful Affair; and fhall think my Time has been well employed, if it can afford you any Satisfaction.

Tho' Fifhes are not provided with Organs for Hearing, fimilar to thofe ferving to that Purpofe in other Animals, it would be too prefumptuous to declare, without Experiment, that they are unable to hear, by Organs differently placed, whofe Situation and Structure, for want of due Examination, we are unacquainted with.

In order therefore to be able to judge from real Facts, without being in the leaft prejudiced by what has been written for or againft their Capacity of Hearing, I have, for almoft three Years paft, been continually trying Experiments on feveral Kinds of Fifhes; *viz.* Perches, Ruffs, Banfticles, Millers Thumbs, Minnows, *&c.* which I have kept in Glafs Jars for that Purpofe; and at the Hours of feeding them, as well as at other Times, have, by different Noifes, fuch as Whiftling, Halloing, the Sounds of feveral mufical Inftruments, and every other means I could contrive, endeavoured to difcover their Senfe of Hearing, if they were indeed endowed with that Senfe; but could never perceive they were affected by any of thefe Noifes.

But whether Fifhes do or do not hear, it is certain their Senfes of *Feeling* and *Seeing* are exquifitely quick; and I believe, by the extreme Senfibility of thefe two, one may explain moft of the Accounts that have been brought by Writers as Proofs of their Hearing; fuch as their coming, when called by their Names, as *Plutarch* relates of *Marcus Craffus*'s Lamprey; their flocking in Throngs

when

when called to be fed, as Mr. *Bradley* tells us he ſaw the Carps do in the Pond of one Mr. *Eden* at *Rotterdam*; and their flying away from the Hallo-ing and Noiſes made by Sailors, as *Wolfgangus* reports the Dolphins do, when the Sailors have a Mind to fright them. — But may we not as reaſonably imagine theſe Dolphins fly from the Sailors, their Ships and Boats, on account of the violent Action wherewith ſuch Halloings uſually are performed, as merely on account of the Noiſe they make? And in the other Caſes, is it not as probable, that the Fiſh in Ponds, either by their Sight or Feeling, diſcover'd the Approach of their Benefactors, whoſe coming they were accuſtomed to expect, as that they were ſenſible of their Voices calling them?

I have often ſtruck with my Thumb-Nail againſt the Edge of a glaſs Jar, in which I kept two Ruffs, a Stroke not harder than the Beat of a Pulſe, which would cauſe them in a Moment to dart from the Bottom of the Jar to the Top; tho' I am ſure they did not ſee me. But if I made the ſame Motion without hitting the Glaſs, or if I made an hundred times louder Noiſes than the ſtriking of my Nail againſt the Glaſs, at a very ſmall Diſtance from it, I could not perceive they were in the leaſt affected thereby; which, if duly conſider'd, may I think amount to a Proof of the Deafneſs or Want of Hearing in this kind of Fiſh at leaſt; and that their delicate Senſe of Feeling ſupplies them with the Knowledge of the Motions of Bodies, when their other Senſes fail. Indeed I have often been convinced by Experiment, that their Feeling is exceedingly acute, perhaps more ſo than in other Animals; whence I have been led

to

to imagine, that their Fins may poſſibly be the Organs more immediately ſenſible of the ſlighteſt Motions in the Medium wherein they dwell. The Curious, who have obſerved the Fins of Fiſhes with the Microſcope, find them to be compoſed of infinitely fine Veſſels, Arteries, Veins, Muſcles, and membranous *Fibrillæ*, whoſe Structure ſeems more delicate than is neceſſary for Parts that ſerve only as Oars to waft the Fiſh along. This however I deſire you to regard as a mere Conjecture, for which the neceſſary Proofs are wanting.

At other times, if, by ſtriking on the Top of the Jar with a ſmall Key, the Stroke or Tremor has been a little more violent, the Fiſh would ſhut down their back Fins in a Moment, and remain motionleſs at the Bottom of the Glaſs. The ſudden Appearance of my Hand at the Top of the Jar would likewiſe produce the ſame Effect; but Noiſes made near them ſeemed to give them no Diſturbance.

Theſe Experiments I have often repeated before ſeveral of my Acquaintance, as well as by myſelf in private, and ſeldom found them to vary much. But Fiſh newly taken out of Ponds, or Rivers, muſt not be expected to perform all I here relate: For they, like Birds juſt taken in the Fields, and put in Cages, are thrown into Confuſion at the Approach of any thing, and endeavour continually to regain their Freedom.

If the Eyes of Fiſhes be carefully examined, when ſwimming in a glaſs Veſſel, the *Cornea* or black *Uvea* of their Eyes may be ſeen, ſometimes advancing forwards, and at other times retiring back, juſt as their Sight is directed to near or diſtant Ob-

jects,

jects, through a groſſer or finer Medium; the Form of their Eyes altering, as the Occaſion requires, to make them diſtinguiſh Objects; and their Eyes have ſo great a Liberty in the Orbits, that they are able to turn them any Way, upwards, downwards, to one Side or the other, nearly a Quarter of a Circle, which makes them full amends for the want of Motion in their Necks, and enables them to change or direct their *Optical Axis* to any deſigned Place in a Moment.

Thoſe who have been accuſtomed to Fly-Fiſhing can bear Witneſs, that the Sight of Fiſhes is quick and diſtinct almoſt beyond Belief: For it is not uncommon to behold a Fiſh dart itſelf 20 or 30 Yards in an Inſtant at a Fly thrown out at the End of a long Line, and catch it even before it can well touch the Water. Few other Creatures are perhaps capable to diſtinguiſh Objects ſo ſmall at ſo great a Diſtance, at leaſt not ſo perfectly as theſe do; for, let the artificial Fly differ in Colour, Shape, or Bigneſs but very little from the natural one it ſhould repreſent, and not a Fiſh will meddle with it.

Theſe Inſtances of the exquiſite *Feeling* and *Seeing* of Fiſhes, together with their Want of Organs that can be certainly known to ſerve them for *Hearing*, as well as of ſufficient Facts to prove that they do hear, may, I think, amount to the higheſt Probability, that they are really deſtitute of that Senſe *, and ſtand in no need thereof, notwithſtanding

* 'Tis not hereby denied, that Fiſhes of the cetaceous Kind may probably hear, as well as ſome other Kinds produced in the Sea, that have Parts in common with Land Animals. Theſe Obſervations are confined to the common Fiſh of our Rivers.

ſtanding the contrary Opinions of ſome Authors: And their living in an Element, where Land Animals are capable of remaining but a very ſhort time, may render an abſolute Certainty in this Caſe unattainable.

But in order to diſcover what Land Animals can do, or what Fiſh, had they Organs of Hearing ſimilar to thoſe of Land Animals, would be capable of doing, I endeavoured laſt Summer to find out by Experiment,

First, Whether or no Sound made in the open Air can be heard by a Land Animal immerged under Water.

Secondly, Whether, and in what Manner, Sound made under Water can be heard by a Land Animal in the open Air. And,

Thirdly, Whether, and in what Manner, Sound made under Water can be heard by a Land Animal that is likewiſe under Water.

To ſatisfy my firſt Inquiry, whether Sound made in the open Air can be heard by a Land Animal under Water; I cauſed three People, ſtript quite naked, to dive down at the ſame time, and to remain about two Feet below the Surface of the Water; in which Situation I ſpoke to them as loud as I was able. At their coming above Water, they repeated my very Words, but ſaid I ſpoke very low.

I cauſed the ſame Perſons afterwards to dive down about 12 Feet under Water, and a Gun was diſcharged over them, which they all ſaid they heard, but that the Noiſe was ſcarce perceivable.

As to my ſecond Inquiry, Whether, and in what Manner, Sound made under Water can be heard

in

in the open Air: I caufed a young Man to dive fome Feet down, and then to endeavour to halloo, which he did; and I could hear him, though very faintly. But imagining the Sound might come up with the Water he difcharged at his Mouth whilft he halloo'd, I contrived a kind of Hand-Granado, which I threw into a Place in the River about nine Feet deep. The Fuzee burnt under Water near 10 Seconds, and then the Granado went off, giving a prodigious hollow Sound, and fhaking the adjacent Ground to fuch a Degree, that the Whole of a large Building, fome Yards diftant from the Explofion, was put into a Tremor, far beyond what could be expected from fo fmall a Quantity of Powder.

I fatisfied my third Inquiry, Whether, and in what Manner, Sound made under Water can be heard by a Land Animal that is likewife under Water, by procuring a young Man to dive down with a Bell in his Hand; and he affured me, that he heard its tinkling very diftinctly, at all Depths under Water, with little or no Difference from what he did when rung in the open Air: He likewife affirmed, that he plainly heard the Noife and Rufhing of the Water, which came violently through a Flood-Gate, about 20 Feet diftant from the Place he then was in.

If thefe Experiments and Obfervations may be thought deferving Notice, I fhall think my Time not thrown away; but at all Events be affured, that I am,

Dear Sir,

Norwich, Nov. 27.
1747. *Your moft obliged humble Servant,*

W. Arderon.

V. *The Substance of some Experiments of planting* Seeds *in* Mofs, *lately made by Mr.* Charles Bonnet, *of* Geneva, *F. R. S.*

Read Feb. 18. 1747-8.

MR. *Bonnet* was inclined to try whether Plants were capable of Vegetation, when they were only fet in Mofs, inftead of being planted in the Earth.

With this Defign, he filled with Mofs feveral Garden Pots, and he compreffed the Mofs more or lefs, as he judged, the feveral Plants he intended to place in them, might refpectively require a clofer or a loofer Soil.

He then fowed in Mofs, Wheat, Barley, Oats, and Peafe. And he found, firft, that all the Grains fowed in that manner came to Maturity later than thofe of the fame Sorts which were fowed at the fame time in Mould.

2dly. That the Stems from the feveral Grains fowed in the Mofs were generally taller than thofe which fprung from the Ground.

3dly. There came from the Grains fowed in the Mofs a greater Number of Blades than from the Grains fowed in the Earth.

4thly. The Grains fowed in Mofs produced more plentifully than the others.

5thly. Thofe Grains that were gather'd, from the Produce of thofe which vegetated in the Mofs, having been again fowed fome in Mofs, and fome in Earth, fucceeded well in both.

Mr. *Bonnet* has alfo planted in Mofs, Pinks, Gillyflowers, Daifies, Tuberofes, Tulips, Hyacinths, Jonquils, and Narciffus's; and all thefe Plants fucceeded as well

well as others of the ſame Sorts, which he at the ſame time planted in Mould.

He alſo placed in Moſs Cuttings and Layers of Vines, and theſe Cuttings and Layers became Vines; and theſe Vines in a ſhort time grew larger than others, that came from Cuttings and Layers planted at the ſame time in the Ground.

VI. *A Continuation of an Account of an Eſſay towards a* Natural Hiſtory *of* Carolina, *and the* Bahama Iſlands; *by* Mark Catesby, *F. R. S. with ſome Extracts out of the Appendix,* * *by* C. Mortimer, *Secret. R. S.*

Read Feb. 18. 1747-8.

1 *URogallus minor, fuſcus; cervice plumis alas imitantibus donatâ.*

This Bird was about a third Part bigger than a common Partridge, all over of a reddiſh Brown, marked tranſverſely with black and white waved Lines intermixed; but what is ſingular and extraordinary in this Bird, and diſtinguiſhes it from all others yet known, are two Tufts of Feathers reſembling little Wings, 3 Inches long, placed on the hind Part of the Head, oppoſite to one another. Theſe little Wings (if ſo they may be called) were fixed, as our Author ſays, to the Neck, in like manner as the real Wings are to the Body; whereby it had the like Power of contracting and dilating them; and they may

* See the Extract of the tenth Set in theſe *Tranſ.* N°. 484, *p.* 599.

possibly assist the Bird, in running, or flying, or both, especially as the Wings are short in proportion to its heavy Body. They are Natives of the Northern Parts of *America*. Lord *Wilmington* had one of these Birds at *Chiswick*.

Meadia. So called in Honour of Dr. *Mead*.

The Leaves of this Plant resemble those of a Lettuce, from which rises a single Stalk, about a Foot high; on the Summit of which are fixed many rectilinear Footstalks, on every one of which hang pendent a single purple Flower, which is monopetalous, somewhat resembling the Autumn *Cyclamen*. This is a very ornamental Plant, and flowers yearly at Mr. *Collinson*'s Garden at *Peckham*.

2. *Scolopendra*. This is a very venomous Insect, seldom found without the Tropics, being most numerous in the hottest Regions. Their offending Weapon is a Pair of *Forceps*, armed with two sharp Points, which meet when they bite, and cause a very acute Pain for eight or ten Hours, abating very gradually. Their Bite is said to be as bad as that of the Scorpion.

Hamamelis. This Winter flowering Shrub was sent to the Author from *Virginia*, and arrived at *Christmas*, full of yellowish Flowers.

3. *Monedula, tota nigra*. Hist. Jam. 298. Vol. 2. The Razor-bill'd Black-bird of *Jamaica*.

The singular Make of the Bill of this Bird distinguishes it from others; the upper Mandible being remarkably prominent, rising arch-wise, with a high and very thin Edge. They appear in numerous

Flights

Flights in *Jamaica* and *Hispaniola* voraciously destroying the Grain there, and in other warm Parts of *America*.

Calceolus, flore maximo rubente, purpureis venis notato; foliis amplis hirsutis crenatis; radice Dentis Canini.

This Plant produces the most elegant Flower of all the helleborine Tribe; and is in great Esteem with the *North American Indians* for decking their Hair, *&c*. They call it the *Mocasin* Flower, which also signifies in their Language a Shoe or Slipper, and is by these *Indians* made very like that ancient Slipper or Shoe lately found in the Isle of *Axholme* in *Yorkshire*, and shewn to the *Society Oct.* 22. last *.

4. *Vespa Ichneumon tripilis Pensylvaniensis.*

Rhus glabrum, Panicula speciosa coccinea. Pensylvanian Sumach.

This *Rhus*, for the Resplendency of its scarlet Panicles, excels all others of the Tribe. The Colour begins to appear in *July*, with a Tincture of Yellow; but as the Fruit ripens, the Scarlet heightens, as appeared by Plants in their full Lustre on the 30th Day of *September* 1747, in the Author's Garden at *Fulham*. The Berries that compose the Panicles were thick-set with numerous Filaments or small Threads of a purple or scarlet Colour (best discerned by a Microscope); which receiving a Reflexion from the Yellow, causes this glorious scarlet Colour, which nothing can excel; more especially when the Sun shines upon it. It is a

* See these *Trans.* N°. 484, *p.* 575.

a Native of *Virginia*, but agrees well with our Climate.

5. *Pica luteo-nigra varia.* The yellow and black Pye. *Hist. Jam.* p. 301.

These Birds in *Jamaica* are called *Bonano* Birds; that Fruit being a Part of their Food. They are very sprightly and active Birds, and are often kept in Cages, for their Docility, and antick Gestures.

Lilio-Narcissus Polyanthes, flore albo. This bulbose-rooted Plant grows plentifully in the boggy Soil of *Georgia*.

Vespa Ichneumon cærulea.

6. *Cacao Arbor.* The Cacao or *Chocolate* Tree.

This excellent Tree is found no-where but in *America*, and there only between the Tropics. 'Tis pity the Culture of this so useful and valuable a Tree should be neglected by us, when the Soil and Climate of all our Sugar Islands is as well adapted to its Growth, as any of the *Spanish* or *French* Territories. Notwithstanding which they supply us and all the World with it. Our Author thinks this deserves the Consideration of the Legislature; for were a Method found to encourage its Cultivation, we might not only supply our home Consumption of *Chocolate*, but come in for a Share of Exportation to foreign Markets.

7. *Volubilis siliquosa Mexicana, Plantaginis folio.* Hist. Jam. 180. Vol. I. The Vanelloe.

With the Fruit of this Plant the *Spaniards* perfume their Chocolate.

8.

8. *Hirundo, cauda aculeata, Americana.* The *American* Swallow.

The Singularity of this Bird is, that the Shafts of the Tail Feathers are very ſtiff, ſharp-pointed, and bare of Feathers at their Ends, which ſeem deſigned by Nature for the Support of their Bodies, while they are in an erect Poſture, building their Neſts; which they do in Chimnies, with little Sticks interwoven and cemented together with a kind of Glue or Gum.

This Bird arrives and retires from *Carolina* periodically, and agrees with the Deſcription of *Murgravius's Andorinha* of *Brazil*; except that he takes no notice of the Spines in the Tail; which he might probably overlook. Could it be aſcertained, that this and *Margrave's Andorinha* were the ſame, it would, I think, confirm that moſt probable Hypotheſis, that Birds of Paſſage (particularly Swallows) paſs in our Winter to the ſame Latitude in the Southern Hemiſphere, as the Northern Latitude, from whence they came.

Lilium anguſtifolium, flore rubro ſingulari. The red *Penſylvanian* Lily. This Lily comes from *Penſylvania.* It agrees with our Climate.

9. *Pomifera, ſeu potius Prunifera Indica, nuce reniformi, ſummo pomo innaſcente Cajous vel Acajous dicta.* *Raii* Hiſt. Cat. Jam. The *Cajou* or *Caſſu* Tree.

This forms a regular-headed handſome Tree, producing beautiful fragrant Flowers ſucceeded by its wholſome and nutritious Fruit, which is alſo of ſingular Structure and Beauty: The Stem of the Fruit

4 in

is in a moſt ſingular manner placed at the Crown of the Fruit on the Outſide of the Fruit itſelf, in Form of a Hare's Kidney, and contains a Kernel of the Size and Taſte of an Almond: The Shell incloſing this Kernel is double, and contains an acrimonious cauſtic inflammable Oil; which, if applied to a tender Part of the Skin, fetches it off. It remains in Linen marked with it, the whole Time of wearing; and is therefore uſed for that Purpoſe in the *Weſt Indies*.

10. *Ardea criſtata maxima Americana*. The largeſt creſted Heron.

This is the largeſt Species of Heron yet known, and is a great Devourer of Lizards, Efts, *&c.*

Stellio aquaticus minor Americanus. The ſpotted Eft.

Pulex minimus, cutem penetrans, Americanus. The *Chego*. This is ſmaller than the ſmalleſt of our common Fleas; they penetrate the Skin, under which they lay a Bunch or Bag of Eggs, which ſwell to the Bigneſs of a ſmall Pea, and give great Pain till it is taken out: To perform which, great Care is required, for fear of breaking the Bag; which endangers a Mortification, and the Loſs of a Leg, and ſometimes Life itſelf.

Scarabæus capricornus minimus, cutem penetrans. This odd Inſect I ſaw (ſays the Author) Governor *Phinney*, of the *Bahama Iſlands*, pick out of his Foot, as he was ſearching for *Chegoes*. It was larger than a common Flea; but magnified to 500 times its Size, appeared as here exhibited.

Blatta

Blatta Americana. The Cock-roach. Theſe are very troubleſome and deſtructive Inſects, and are ſo numerous and voracious, that it is impoſſible to keep Victuals of any kind from being devoured by them, without cloſe covering. They are flat, and ſo thin, that few Cheſts or Boxes can exclude them: They eat not only Leather and Parchment, but Linen and Paper: They diſappear in Winter, and are moſt numerous in the hotteſt Days in Summer, and commit their Depredations moſt at Night.

Blatta maxima fuſca, peltata, non alata. So called from having a ſort of Shield over their Head.

Scarabæus peltatus. This Beetle has its Name for the ſame Reaſon.

11. *Scarabæus pilularis, Americanus.* Tumble-turds.

This is the moſt numerous and the moſt remarkable of the Beetle-Kind of any in *North America*: The Employment they are always ſeen at, is providing *Nidi* for their Eggs; for which they are endowed with the like Sagacity as the Turkey-Buzzard *, to find out their Subſiſtence by the Excellency of their Noſes, which directs them in Flights to Excrements juſt fallen from Man or Beaſt, on which they inſtantly drop, and fall unanimouſly to work, in forming round Pellets thereof, in the middle of which they lay an Egg. Theſe Pellets in *September* they convey three Feet deep in the Earth, where they lie till the Approach of Spring, when the Eggs become animate, burſt their Neſts, and find their Way out of the Earth. Mr. *Catesby* ſays, he has

 attentively

* See this *Hiſt.* of *Carolina*, Vol. I. p. 6.

attentively admired their indefatigable Induſtry, and mutual aſſiſting each other in rolling theſe globular Balls to the Place of their Interment. This they perform Breech foremoſt, by raiſing up their hind Part, and forcing along the Ball with their hind Feet. Two or three are ſometimes engaged in trundling one Ball; which often meeting with Impediments by the Unevenneſs of the Ground, is deſerted by them; yet by others is again attempted with Succeſs; except it rolls into a deep Hollow or Chink, where they are neceſſitated to leave it; repeating the like Action with the next Ball that falls in their Way. No one ſeems to know his own Ball, but an equal Care for all ſeems to affect the whole Community. So intent are they at their Work, that tho' handled, or otherwiſe interrupted, they perſiſt in their œconomical Employment without Fear or Apprehenſion of Danger. The Size of this Inſect is that of the Figure here exhibited; it is all over of a dusky Black; it has ſix Legs, two joined to the *Thorax*, and four to the *Abdomen*.

There are always accompanying theſe abovemention'd ſome larger ones, of a more elegant Structure and Colour, which are much leſs numerous, being about one in twenty to the other. The *Thorax* of this is cover'd with a Shield, of a crimſon-colour'd metallic Luſtre; the Head of the like Colour, blended with Green; on the Crown of the Head ſtands a ſhining black Horn, recurved backward, *&c.* Theſe are commonly called King-Tumble-turds; tho', by what appears, they aſſume no Pre-eminence; but, without Diſtinction, partake of the like dirty Drudgery with the reſt.

Lilium,

Lilium, ſive Martagon *Canadenſe, floribus magis flavis non reflexis.* The *Canada* Martagon. Theſe Plants have flower'd ſeveral Years in Mr. *Collinſon's* Garden at *Peckham.*

12. *Perdix ſylveſtris, Virginiana.* The *American* Partridge.

This is about half the Size of the *Perdix cinerea,* or common Partridge, but much more elegantly colour'd. Theſe, contrary to ours, are not often found in open Fields, but moſtly frequent Woods, and ſhady Swamps; their Fleſh is remarkably white, and well-taſted, but of a different Flavour from ours. When raiſed, they perch on the Boughs of Trees.

Lilio-Narciſſus, Virginienſis. Park. The *Attamuſco* Lily. This Plant is a Native of *Virginia* and *Carolina,*

13. *Steuartia.* This elegant Shrub is ſo called in Compliment to the Earl of *Bute,* whoſe Family Name is *Steuart.* It flower'd in the Author's Garden at *Fulham* in *May* 1742. It is nearly akin to the Shrub-Mallows.

Regulus criſtatus. The creſted Wren. It is very remarkable, that this being the ſmalleſt of all our *Engliſh* Birds, is alſo found in *America.*

Veſpa Ichneumon, of a yellowiſh brown Colour.

14. *Avis Tropicorum.* The Tropic Bird.

The Name of theſe Birds ſeems to imply the Limits of their Abode; for they are not often ſeen much North or South of the Tropics; yet are they ſeen all over the Ocean within thoſe Limits, from

the Continent of the Old to the New World, and are very remarkable and different from all other Birds, in having a Tail confifting only of two very long narrow Feathers. The whole Bird is white, except the Bill and Legs and Feet, which are red, and about the Eyes, and near the Tips of the Wings are Spots of Black.

Larus minimus, marinus, naribus tubulatis. The Pittrel or Storm-Fink. This is a Sea-Bird, no bigger than a Sparrow, and is remarkable for being the fmalleft of all Birds that are web-footed. Their Appearance is generally believed by Mariners to prognofticate a Storm, or bad Weather. They ufe their Wings and Feet with furprizing Celerity: Tho' their Feet are formed for Swimming, they are likewife fo for Running, which Ufe they feem to put them to; being ofteneft feen in the Action of running fwiftly on the Surface of the Waves in their greateft Agitation, but with the Affiftance of their Wings. The Author has feen them ofteneft in bad Weather.

15. *Magnolia, flore albo, folio majore acuminato haud albicante.*

This is the fourth and laft-difcovered Species of that elegant Tribe of Trees the *Magnolia.* Some Seeds of it with Specimens were fent me from the only Tree of it known in *Virginia.* Its majeftic and elegant Appearance excites many People far and near to vifit *Kit Smith*'s Tree; that being the Name it has attain'd, and is known from the Name of the Man in whofe Land it grows. This Tree, tho' fcarce in *Virginia*, has been fince found to

grow

grow in great Plenty in the North-Weſt Parts of *Penſylvania*.

Formica villoſa coccinea. The Velvet-Ant. This Inſect is ſhaped like an Ant, and is about the Size of a Hornet. The Body is elegantly marked with black and crimſon Velvet. The *Thorax* is of ſo ſtrong and hard a Contexture, that, being trod upon by Men or Cattle, they receive no Harm. They have a long Sting in their Tails, which cauſes Inflammation and great Pain to thoſe who are ſtung.

16. *Caprimulgus minor Americanus*. The Whip-poor Will.

This nocturnal Bird is about the Size of a Blackbird. It has the ſmalleſt Bill, and wideſt Mouth, of any other Bird, in proportion to its Size; it hides itſelf in the Day, and is then never ſeen, but at the Dusk of the Evening he ſets up his Cry, repeating it inceſſantly, till Break of Day, making a very loud and ſhrill Noiſe, which, the Echoes from the Rocks, and Sides of Mountains, increaſe to ſuch a Degree, that the Silence of the Night is much interrupted thereby. Their Cry is like the Sound of the Pronunciation of the Words *Whip-poor-Will*.

The *Indians* ſay, theſe Birds were never known till a great Maſſacre was made of their Country Folks by the *Engliſh*; and that they are Souls or departed Spirits of the maſſacred *Indians*. Abundance of People look upon them as Birds of ill Omen, and are very melancholy, if one of them happens to light upon their Houſe, or near their Door, and ſet up his Cry (as they will ſometimes

upon

upon the very Threſhold). Such are the Superſtitions of theſe ignorant People.

Aureliana Canadenſis. R. P. Lafiteau. The *Ginſeng*, or *Ninſin* of the *Chineſe*.

Ginſeng is the Root of a Plant of the higheſt Eſteem with the *Chineſe* for its medicinal Virtues; and many Volumes have been wrote by their moſt celebrated Phyſicians, to illuſtrate its wonderful Effects. Tho' moſt of the Writers of *China* take notice of the *Ginſeng*, yet it was little known, till Father *Jartoux*, a Jeſuit and Miſſionary in *China*, who being employed by Order of the Emperor of *China* in making a Map of *Tartary* in the Year 1709. had an Opportunity of ſeeing it growing on the Confines of the Kingdom of *Corea*. That Father took an Opportunity to make a Draught of the Plant, and give an accurate Deſcription thereof; which being publiſhed in the Memoirs of the *Academy* of *Sciences* at *Paris*, gave Light to the Diſcovery of the ſame Plant in *Canada* and *Penſilvania*; from which laſt Place it was ſent to Mr. *Collinſon*, in whoſe curious Garden at *Peckham* it has the preceding, and alſo this Year 1746, produced its Bloſſoms and Berries, as it appears in the Figure here exhibited, and agrees ſo exactly to the Father's Deſcription of the *Chineſe Ginſeng*, that no Doubt can be made of its being the very Species he deſcribes. But as the Jeſuit's Account is too long to be inſerted the Author has only given an Abſtract of it, and added to his Figure the Bloſſoms which the Father owns he never ſaw.

This concludes the whole Number of Birds exhibited in both Volumes, containing in all

113; in which are alſo contained all the Land Birds Mr. *Catesby* ever ſaw or could diſcover in that Part of *North America* included between the 30th and 45th Degrees of Latitude. And tho' more Kinds may not improbably remain unknown within thoſe Limits, yet North of them he thinks there cannot reaſonably be thought to be many new Species; becauſe there are not only but a few Birds at the Northern Limits, but alſo becauſe Animals in general, and particularly Birds, diminiſh in Number of Species, as they approach the Pole.

17. *Chamærhododendron Lauri folio, ſemper vireſcens, floribus bullatis corymboſis.*

This Tree riſeth to the Height of about ſixteen Feet, producing ever-green Leaves, in Shape like the *Lauro ceraſus*, of a ſhining Dark-green; the Flowers grow in Bunches, the Bud or Rudiment of which appears in Autumn wrapped up in a conic ſcaly *Perianthium*, on which is a viſcous Matter, which protects them from the Severity of the Cold in Winter. In the Spring theſe Buds break forth into monopetalous bluſh-colour'd Flowers, with ſome of its Petals ſpotted with yellow, green, and purple. The whole Plant is of a moſt elegant Appearance: Its native Place is *Penſilvania*.

Chamædaphne ſempervirens, foliis oblongis anguſtis, foliorum faſciculis oppoſitis e foliorum alis. This Plant is a Native of *Penſilvania*, but has flower'd at *Peckham* in *September* 1743.

18.

18. *Lepus Javensis.* The *Java* Hare.

It is about the Size of an ordinary Hare; the Head ſmall, in proportion to the Body; the Eyes large and prominent; the Ears like thoſe of a Rat; except which, the Head partook of a Likeneſs both of a Deer and a Hare; the hind Part of the Body remarkably big.

Ficus, Citri folio, fructu parvo purpureo. This is one of thoſe kind of Trees mention'd by *Q. Curtius, lib.* ix. *c.* 1.

19. *Vipera marina.* The Viper-Mouth.

This Fiſh was 18 Inches long: But as Fiſh are not (as Quadrupeds) of a determinated Size, ſo theſe are ſaid ſometimes to grow to a vaſt Bigneſs. The Mouth was exceſſive wide: Both Jaws were armed with ſharp deſtructive Teeth; particularly two in each Jaw were much longer than the reſt, ſo that they could not be admitted within the Mouth. Moſt of theſe long Teeth had an angular bending towards their Ends in a very ſingular manner. It was without Scales, mark'd all over with hexagonal Diviſions. This Fiſh was of the oddeſt Structure, and moſt formidable Appearance, of any the Author ever ſaw. It came from *Gibraltar*, and was taken in the Harbour there, and is now in Sir *Hans Sloane*'s *Muſeum.*

Cataphractus Americanus. The Armour-Fiſh. This Fiſh was ſomewhat leſs than a Foot in Length, and four Inches broad; a ſmall Part of the Belly was cartilaginous; except which the whole Fiſh was cover'd with hard thick Bone, but in a different manner; *viz.* the Head and fore Part of the Fiſh

was

was alſo cover'd with Plates of Bone, extending from the Back to the Belly, and lapping one over another. It was armed with three ſtrong pointed Bones, thick-ſet, or rather ſerrated with Teeth, one placed near the Back, and one near each Gill. Theſe Bones were three Inches long, and ſo fixed in Sockets, that the Fiſh can point them to any Direction, in Defence of itſelf. This Fiſh having no Teeth for Defence, Nature ſeems to have compenſated that Deficiency, by beſtowing on him Weapons and Armour in a very extraordinary manner. It was given by Captain *Wm. Walker, F. R. S.* to Sir *Hans Sloane.*

20. *Biſon Americanus.*

This is the only Species of the wild Cow-kind that is known in *North America*; there being none of our Cow-kind there, till brought over from *Europe*. They are low of Stature, but weigh more than our largeſt Oxen: The Skin of one is too heavy for the ſtrongeſt Man to lift from the Ground: Their Limbs are large, their Cheſts broad, as are their Heads; their Horns are large at their Baſis, and turn inward; on their Shoulders is a large Prominence or Bunch; in Winter their whole Body is covered with long ſhaggy Hair, which in Summer falls off, and the Skin appears black and wrinkled; except the Head, which retains the Hair on all the Year. On the Forehead of a Bull the Hair is ſo long, that, by hanging over his Eyes, it impedes his Flight, and is frequently the Cauſe of his Deſtruction. But this Obſtruction of Sight is in ſome meaſure ſupplied by his good Noſe, which is no ſmall Safeguard to

him.

him. A Bull in Summer, with his Body bare, and his Head muffled with long Hair, makes a very formidable Appearance. They range in Droves, feeding in open *Savanna's* Morning and Evening; and in the sultry Time of the Day they retire to ſhady Rivulets, and Streams of clear Water, gliding through Thickets of tall Canes; which, tho' a hidden Retreat, yet their heavy Bodies cauſing a deep Impreſſion of their Feet in moiſt Land, they are often traced and ſhot by the artful *Indians*. When wounded, they are very furious; which cautions the *Indians* how they attack them in open *Savanna's*, where no Trees are to ſcreen them from their Fury. Their Hoofs more than their Horns are their offenſive Weapons; and whatever oppoſes them are in no ſmall Danger of being trampled into the Earth. Their Fleſh is very good, of a high Flavour, and differs from common Beef, as Veniſon from Mutton. The Bunch on their Shoulders is eſteemed by the *Indians* the moſt delicate Part of them.

Pſeudo-Acacia hiſpida, floribus roſeis. The Flowers and Leaves of this Tree differ little in their Shape from the *Pſeudo-Acacia flore albo.* The Stalks and larger Branches are thick-ſet with prickly Hairs, and with ſharp Spines, placed alternately: The Flowers, which are papilionaceous, are of a faint purple or roſe Colour, and of a fragrant Smell. I never ſaw any of theſe Trees but at one Place near the *Apalatchian* Mountains, where Buffaloes had left their Dung, and had been brouzing on the Leaves. What with the bright Verdure of the Leaves, and the Beauty of the Flowers, few Trees make a more elegant Appearance.

Thus

Thus ends the moſt magnificent Work I know of, ſince the Art of Printing has been diſcover'd: The Deſcriptions are all given in both *Engliſh* and *French*; and the Figures being drawn by the ingenious Author after Life, were afterwards etched by himſelf, and all the illuminated Sets were colour'd under his Directions, and all touch'd up and finiſh'd by his own Hand.

VII. *The Inſcription upon a* Roman *Altar found near* Stanhope *in the Biſhoprick of* Durham; *communicated to the* Royal Society *by the Reverend Mr.* Thomas Birch, *F. R. S.*

Read Feb. 18. 1747-8.

SILVANOINVICTOSACRVM
CTETIVSVETVRIVSMICIA
NVSPREFAIAESEBOSIAA:
NAEOBAPRAMEXIMIAE
FORMAECAPTVMQVEM
MVLTIANTECESSO
RESEIVSPRAEDARI
NONPOTVERVNTVSLP

Silvano invicto ſacrum
C. Tetius Veturius Micianus, Præf. Alæ Seboſianæ, ob Aprum eximiæ formæ captum quem multi Anteceſſores ejus prædari non potuerunt Votum ſolvens lubens poſuit.

VIII. *A Letter from Mr.* Henry Baker, *F. R. S. to the* President, *concerning an extraordinary* Fish, *called in* Russia QUAB; *and concerning the Stones call'd* Crabs-Eyes.

Read Feb. 25. 1747-8.

HAVING been invited some time ago to a Correspondence in *Muscovy*, with Dr. *James Mounsey*, one of the Physicians to the *Czarina's* Armies, a Gentleman much esteemed in that Country, for his Knowledge in Natural Philosophy, and his unwearied Endeavours to discover Truth, I readily embraced so favourable an Opportunity of making Inquiry concerning some Things, as to which the Accounts hitherto received appeared to me extremely doubtful.

I therefore desired of the Doctor to send me what Information he could depend on, first, concerning the Swallows, and other Birds of Passage, that are observed in *Russia*, as we have had some Accounts of them that seem incredible. Secondly, concerning an extraordinary Fish in that Country, called the *Quab*, which is reported to be first a Tadpole, then a Frog, and at last a Fish. And, thirdly, I requested of him to inform me concerning the *Oculi Cancrorum*, commonly called *Crabs Eyes*, particularly as to their Production, and the Manner of their being gather'd.

In Answer to these Inquiries, I was favoured by the Doctor with a most obliging Letter, accompanied

nied by that remarkable Caſe of a *Fœtus* extracted by him from one of the *Fallopian* Tubes, where it had been lodged 13 Years (ſee this *Tranſ.* p. 131.). And I now beg Leave to lay before you ſome Extracts from this Letter, as Matters not only of Curioſity, but of conſiderable Moment alſo, in the Natural Hiſtory of Animals.

He deſires I'll allow him another Year to perfect his Obſervations on the Birds of Paſſage; being unwilling, he ſays, to rely on the Accounts of others, where he can come at the Knowlege of Things himſelf.

As to the *Quab*, which ſome report to be firſt a Tadpole, then a Frog, and at laſt a Fiſh, 'tis very well known, he ſays, to him; but with regard to ſuch Changes, he believes them to be intirely fabulous. He has indeed ſeen, in the Chamber of Rarities at *Petersburg*, this Fiſh, preſerv'd in Spirits, under all theſe Appearances; but was not permitted to take out any one of them, in order to remove the Scruples he made: However, deſiring as far as poſſible to come at the Truth, he turn'd the Bottle haſtily on one Side, to make the Fiſh fall to the Glaſs, which he thought they did, with more ſeeming Hardneſs than could be ſuppoſed in Fiſhes; which induced him to conjecture, that they are Pieces of Art, the Idea whereof has been taken from the Reſemblance of the Head of this Fiſh to that of a Frog. Whence he ſuppoſes they may be made of Wax, and kept in this manner to amuſe the World. If there be, he ſays, ſuch a Thing in Nature (which he does not think probable) it muſt be peculiar to ſome one Place, whereof he has no

 Knowlege.

Knowlege. He has made Inquiries about theſe imagin'd Changes, of People of many Nations, but could never learn any thing to the Purpoſe. He has ſeen the Fiſh itſelf in ſeveral Countries, and found they ſpawn'd like other Fiſhes, and grew in Size, without the leaſt Similitude to what has been aſſerted. He adds further, that theſe Fiſhes delight in very clear Water, in Rivers with ſtony or ſandy Bottoms, and are never found in ſtanding Lakes, or Rivers paſſing thro' marſhy or moſſy Grounds, where Frogs chuſe moſt to be.

As to my Inquiries concerning the *Crabs-Eyes*, he expreſſes a Surprize to find Naturaliſts differ ſo much from one another, and yet not one of them he has ever ſeen giving any true Account of the Situation, Formation, and caſting of theſe concreted Bodies. He therefore is ſo obliging to ſend me the following Deſcription from his own Obſervation and Knowledge.

Thoſe Concretions called Crabs-Eyes, are found, ſays he, in the Bodies of Cray-fiſh. Each Fiſh annually produces two, one on either Side of the anterior and inferior Part of the Stomach, and each is generated about a Point lying between the Coats thereof. The flat or concave Side lies next the internal Coat, which is very thin and clear, though ſtrong and horny; the convex Side is conſequently outwards, and is immediately cover'd by the fleſhy and ſofter Coats of the Stomach, whoſe Fibres make Impreſſions on its Surface. Between theſe two Membranes it grows by degrees *lamellatim*, and is ſupplied with petrifying Juices diſcharg'd through the Mouths of Veſſels or *Sudamina* opening on the internal

nal Surface of the outer Coat. The inner Membrane, being horny, gives Reſiſtance only; wherefore the Stones are concave on that Side, and the firſt remarkable Scale (whereon all the others are formed) may be perceiv'd in the Centre, the Brims or Circumferences of many of the reſt being very apparent. At the time theſe Stones are not to be found in the Animal, there are little circular Spots, ſomewhat opake, and whiter than the reſt of the Stomach, to be perceiv'd in their Place; nearly oppoſite to which are tenacious mucilaginous Subſtances, form'd like little *Placentulæ*, and call'd by ſome the Glands of the Brain: Theſe are larger, and more perceptible when the Stones are wanting; but are not turned into Stones by different Degrees of Induration, as ſome have imagined them to be.

It is believed, he ſays, that they caſt theſe Stones with their Shells, which they ſhed every Spring; but he finds this is not the Way of getting rid of them; for, a little before, or after the Time of their caſting their Shell, the Stones break thro' the internal or horny Coat of the Stomach, and being ground or broken by the three ſerrated Teeth therein, become diſſolv'd in the Space of a few Days, which makes it difficult to find them juſt at this time, and thereby gives Ground to imagine they are caſt with the Shells. He ſays, however, he has found ſeveral of them in the Stomach partly conſumed, one Specimen whereof he has ſent herewith, and a farther Proof that they are ſo conſumed, is, he thinks, their being never diſcover'd in Rivers, tho' the Fiſh themſelves be in great Plenty there; and in the Shops it is obſervable, that many of theſe Stones are of a brown

brown Hue; which is the Cafe of fuch as have been already lodg'd in the Cavity of the Stomach, when the Fifh was taken. They likewife eat the old Shells immediately after fhedding them *. What the Ufe of thefe Stones to the Creature is, he cannot pofitively determine, but fuppofes they may be defign'd to furnifh new petrefcent Juices to its Fluids; which may be alfo affifted by the old Shells which they devour, the Particles whereof, as well as of the Stones are probably difpos'd of, according to their Degree of Purity, and properly depofited at the Extremities of Veffels, for the Reproduction of their annually new crufty Drefs; which, he obferves, does not greatly recommend the Opinion that thefe Stones have a diffolving Quality, of Service againft the Stone in the human Kidneys or Bladder.

The Doctor has fent along with this particular Account, Specimens of the Cray-fifh both boil'd and raw, which differ little or nothing from thofe catch'd in our Rivers here; in which I am affured the like Concretions may be alfo found at a certain Time of the Year: He has likewife fent me fome of their Stomachs dried, where the Stones appear, fituated in the manner above defcribed between the two Coats; and in one of them they are got thro' the internal Coat into the Stomach itfelf. I received alfo from him feveral Specimens of the beginning Scales,

* I have obferved the fame thing in the fmall frefh Water Shrimp; which I have kept in a Glafs with Water throughout feveral of the Periods of its cafting its Shell, which it does once in about a Month or five Weeks. The Water Newt alfo eats its Skin as foon as pull'd off, if it be not taken away.

Scales, or Concretions, of different Bignefs, which he collected himfelf, in diffecting thefe Creatures; feveral of the formed Stones of his own taking out, fome of a larger Size, which were given him by a Gentleman, who took them out of the Cray-fifh in the River *Donne*, and others ftill larger, which he chofe from the Apothecariry of the Army. Thefe laft were from *Aftracan*; and he obferves that the Fifh and Stones are much the largeft in the great Rivers there, where there are Fifhers for Cray-fifh on account of the Stones only; which they feparate from the Fifh at different Fifheries after different Manners; at fome they are beaten to Pieces with wooden Peftles; then wafhing away the Flefh and Shells, the Stones are found remaining at the Bottom of the Veffel; at others they are laid in Heaps till they rot; and then, being wafh'd, the Stones are eafily feparated and gather'd. The Price comes to a Groat or Sixpence a Pound. All the Apothecaries Shops throughout the whole *Ruffian* Empire are furnifhed with them, and great Quantities befides are exported.

This, Sir, feems to be a very particular and exact Account of thefe Productions, which are frequently prefcribed in Medicine. Their Price, we find, is extremely low in the Countries where they are gather'd; notwithftanding which, fictitious Bodies, made of Chalk, Tobacco-pipe Clay, or other fuch-like Materials, caft in Moulds, fo as to reprefent

real Crabs-Eyes, are often ſubſtituted inſtead thereof. Permit me to ſubſcribe myſelf with all poſſible Reſpect,

S I R,

London, Feb. 25. 1747-8. *Your moſt obedient humble Servant,*

Henry Baker.

IX. Brevis hiſtoria naturalis, ſive de Vita, Genere, Moribuſque *Muris Alpini*: Autore *Jacobo Theodoro Klein*, Reipubl. *Gedanenſ.* à Secretis, et R. S. *Lond.* S. communicata per *Petrum Collinſonum*, ejuſdem Societatis S.

Read Feb. 25. 1747-8. BESTIÆ ex murium gliriumve genere ad *animalia induſtria* numerantur. Nulli beſtiarum, nedum inſectorum multitudinibus, ingenitam denegamus induſtriam, *i. e.* mores et ſtudia, pro vitæ ſuæ genere, ad conſervationem et progeniem adæquata. In quibuſdam circumſcripta nobis videntur; uti juxta *aranearum* tribum, ſive ex telis, ſtaminibus, retibus vel caſſiculis ſcientifice conſtructis, ſive ex latebris victum capientium; quædam *philomuſos* dixeris, ut admirandum animal *Caſtorem* (*a*), lignationis, hydrographiæ, aggerationis et architecturæ

(*a*) Conſ. *quadr. Hiſt. prodrom.* p. 19.--- *Hiſt. de l'Acad. des Scienc.* 1737. p. 10. ibid. On ne trouve guere parmis les grands Animaux, que les *Caſtors*, qui ayent une de ces induſtries ſingulieres et incomprehenſibles a l'Eſprit humain.

recturæ ſtudioſum; nonnulla pharmaceuticam et geometricam artem edocta, ut *Apes*; rurſus alia ad artes mere liberales: Petauriſtæ, ſerrarii, naviculatoris (*b*) ut *Sciuri*, ſe profitentia; ne dicamus de inſidiatoribus, prædatoribus, ut *Baviis*, et ſicariis, latrocinia et abactionem exercentibus, *Urſo*, *Lupo*, *Vulpe*, *Muſtelis*; ſic nemo facile animalibus *induſtriam*, pro vitæ ſuæ genere, abjudicaturus.

Sed valde fallimur, fallimuſque aliquando, circa mores nonnullorum, quos ingenitæ induſtriæ ſuperaddimus, multoties cerebrinos. Errores nobis ipſi tribuamus, quod ſæpiſſime hiſtoriolas ex antiquitate ad nos transfuſas æqui boni que habemus, quæ vero ad rectæ rationis examen revocatæ in fabulas abeunt.

Ecquis, emuncti naſi, cum mundo ſymbolico crederet, Leænam catulos edere inanimatos, rugitu matris vel patris animandos? Quis informem maſſam ſive molam *Urſi*, lambendo a matre in ſui ſimilem beſtiam efformandam? Quis *angues*, quos aſtutia et malitia omnes beſtias antecellere dicimus, e cavernis, narium cervi ſpiritu evocandos, ut ſe devorari patiantur? Quis fide dignus teſtis centeſimum *Cervi* ſuperavit annum, quo hujus lacryma ipſa oſſibus ad oculi canthum accreverit ea duritie, quæ cornu ſuperaret? Quis cum Sophocle *lacrymas Gallo-Pavonis* vel *Meleagridis* (*c*) verteret in *Succinum*? Et ſexcenta commenta reliqua.

Quod

(*b*) *Phil. Tranſ.* N°. 427. p. 38.

(*c*) Conſ. *Franc. Vetez. Hiſt.* de los animales, *p.* 70. *Sophocles* entre los antiquos, dixo, que et *Succino* era *lagrimas congeladas* de unas aves llamadas μελεαγρίδες de los Griegos, las quales ſon un genero de Gallinas de las Indias o Moriſcas.

Quod ad MURES ALPINOS attinet; nomen habent a ſummis *Rhetiæ* Alpibus, herbarum graminiſque fertilibus, in quibus habitant. In Sabaudia audiunt *Marmotæ*, in Germania **Murmel Thiere**, in Ukrania, Podolia in montibus Scepuſienſibus, necnon in Ruſſiæ Palatinatu ad pagos Podicemne et Zimnawoda *Bobaſci* (*d*) ad montes vero Carpaticos *Swiſzez*, unde vallis Swiſzeza denominatur, Gallis, *Rats des Alpes*.

Fele domeſtica corpulentiores ſunt; *juniores* rufi; *adulti* ſubfuſci coloris; *pilis* rigidiuſculis; *pedibus* brevioribus; *capite* contracto; *naſo* quaſi diviſo; *ore* felinis pilis ſtipato; *dentibus* ſciuri, vel potius fiberinis; *collo dorſoque* latis et obeſis; *auribus* decurtatis, quaſi mutilis, rotundis; *oculis* ſpecioſis, prominentibus; *cauda* adinſtar caudæ muris avellani, quaſi compreſſa et piloſa, palmam cum dimidio longa; *digitis unguibuſque* ſciuri æmulis, ſed fortioribus.

In *libertate* fructibus, herbis, gramine, radicibus, imo et inſectis vaginipennibus ac locuſtis veſcuntur; *cicurati* variis obſoniis, inprimis lacteariis (*e*) delectantur; carnes, panem, fructus, ſimilemque paſtum, ut ſciuri, anterioribus pedibus ori admoventes.

Catulos ponunt tres vel quatuor; ab *autumno* in verna uſque tempora, per familias juncti, in cavernis ad figuram literæ Y excavatis, et bene obturatis, ſatis ample fornicatis, *alto ſomno* inter tantillum ſtraminum vel fœni ſepulti quieſcunt; ſole rurſus propitio

(*d*) *Rzaczynski*, in auctario Hiſt. Nat. *Polon.* p. 327. quod nondum publice proſtat. (*e*) Murmuratum edunt, dum morſu bibunt lac: Unde *Germanis*, me judice, *Murmel Thiere*; i. e. *murmurans Beſtia*.

pitio *evigilant*, vigilantque clauſtra ſolventes, et ad *egeſtionem* fæcum, ad *paſtum*, et ad opera *aphrodiſiaca* exeuntes . *colludunt* petulanter, uti ſolent *mures* inprimis campeſtres, *ſaltantes* et in arborum *truncos* ludibundi *ſcandentes*; aliquando *bipedes* ingrediuntur; vocem catelli vel acutum fiſtulæ ſonum edentes.

Tardiſſima ipſis eſt per hyemem *ſanguinis circulatio*; tardiſſimæ omnes ſunt corporis *ſecretiones*; *nulla* interim *ſeri* vel lymphæ *revectio*, ut tandem ſanguis omni pene ſero orbetur (*f*). *Omentum* et vicina *interanea* admodum ſunt obeſa. *Ruminantes non* dixeris; *ventriculum* enim ſimplicem habent, membranoſum, licet in libertate conſtituti herbivori ſint; verſus inteſtinum *cæcum valvulas* obſervaveris *conniventes*, *annulares*, et veluti in ramos protenſas, prout ingreſſus *Ilei* inter duas tunicas annulares; ita, ut *nullus* prorſus concedi poſſit *excrementis*, pro egeſtione, ad inteſtina tenuia *regreſſus*, quorum tamen collectio per integram hyemem (qua dormiunt) fit, quæ amandantur ad *cæcum*, ibique ad vernum uſque tempus perdurant.

Hoc vitæ genus eſt, illi ſunt mores, quos exploratos habemus *Murium Alpinorum*, qui BOBAKI vocantur. Quid per *Emptram*, murem montanum, intelligant *Albertus* et *Agricola*, me latet; cum mure autem Norvagico, *Lemmus*, *Leming*, *Lommer* dicto, *Mures Alpini* nihil commune habent; illos

Wormius

(*f*) *Philoſ. Tranſ.* N°. 397.

Wormius (*g*) in nubibus generari vult, exinde in Norvagiam deciduos, qui tamen et in Lapponia ſunt frequentes.

Moribus Marmotarum ſuperaddidit *Plinius*, ſed notanter *ex relatione aliorum*, prorſus ſingularia :—— " Sed ſi pabulo ante in ſpecus convecto, cum quidam " narrent, alternos marem et fæminam ſupra ſe com- " plexo faſce herbæ ſupinos, cauda mordicus appre- " henſa, invicem detrahi in ſpecum, ideoque illo " tempore detrito eſſe dorſo." Hanc narrationem vertit *Agricola* in hiſtoriam Marmotæ naturalem : " Mira vero, inquit, eis machinatio et ſolertia, cum " fœnum jam congeſſerunt ; unus enim humi ſtra- " tus erectis pedibus omnibus jacet in dorſo, in quem " tanquam in plauſtrum quoddam cæteri ea, quæ " congeſſerant, conjiciunt, et ſic onuſtum cauda " mordicus comprehenſa in ſpecum trahunt, et quaſi " quodammodo invehant." Cui *Geſnerus* non videtur aſſentire, inquiens : Eodem modo Caſtorem ligna advehere, et Taxos effoſſam terram pro domiciliis, donec ſatis ampla ſint, vehere, apud recentiores legimus (*h*). Fides penes autores eſto : Facetiam quoque

(*g*) In muſeo ſuo — et in peculiari tractatu, cui titulus : Hiſtoria animalis, quod in Norvagia quandoque e nubibus decidit. *Hafniæ* 1653, 4*to*. Conf. *Scheffer*. Lapponia, c. 29. Hamb. Beytr. 1741. X St. *Linn*. act. Suec. et Fauna Suec.

(*h*) Notum potius teſtatumque facimus : *Caſtores* xyloromos, non per terram ſarraco, ſed per flumina, infixis in ligna ad littora cæſa dentibus ſuis, lignationes pro palatiis advehere. *Rzaczynſkci* p. ~~m. olim hæc de re me conveniens reſponſum tulit~~ : Noli figmenta comprobare. Fuit apud nos vir militaris ſtrenuus venator, qui omni animo, invocatis cunctis divis, nobis perſuadere volebat, quod *Vulpes* terram in cryptis inverſæ caudæ imponat, eamque retro gradus extra antrum, auſſerhalb dem Bau, deponat. Sed fabula manet.

quoque hominis induſtrii et fictum appellare *idem.* Solere hos mures fœnum in dorſo geſtare, cauda per dorſum reducta, et mordicus apprehenſa (*i*) obfirmatum, pro fune ſcilicet, ut homines fœnum bajulant.

Miramur, doctiſſimum *Spon* ſuprafatam narrationem ex animo pro vera venditaſſe hiſtoria: " Rats " des Alpes faiſant *proviſion l'été pour l'hyver* du " foin et d'autres herbes, qui leur ſont neceſſaires, " pour s'en acquiter plus promptement, il y en a un, " qui ſert de *Charrette*, ſe mettant ſur le dos, les " pattes en l'air, et embraſſans le foin. et un autre, " qui ſert de *Charretier*, et le tire par la queûe " juſqu' à leur tanniere; ce qui eſt cauſe, qu' on leur " ordinairement le dos tout pelé (*k*).

Magis miramur noviſſimam relationem, *magni*, et poſt fata, *nominis* CARDINALIS POLINIACI Mures Alpinos bella inter ſe gerere, trucidare ſe invicem belligerantes, et victores victos ſecum ducere in ſervitutem ad ſuſtinenda ſervitia domeſtica, et pro invehendis rebus ad victum neceſſariis (*l*) mores ante a nemine obſervaros; quos ſi *Eminentiſſimus Purpuratus*

(*i*) Cum tamen muribus *Alpinis* ſit caudæ nimis curta ſupellex.

(*k*) Et fœnum et herbæ recentes in antris putredinem hyeme contraherent. Cæterum obeſitas dorſi, petulantia et ſomnolentia raritatem pilorum in dorſo marmotis, et feris et cicuratis, conciliant. Nonne et alia quadrupedia (fere omnia piloſa) capillorum defluvia patiuntur?

(*l*) In *Antilucretio*, lib. vi. verſ. 185. ſeq. (*Pariſ.* 1747, 8*vo.*)

(*Baubaces* patria dixerunt voce Poloni)
Vulpinum genus —
———— de latebris et gramine certant.
—In captivos dominorum provida miras
Sævitia exercet pænas, moeſta agmina condunt
In foveis, coguntque omnes ſervire per annos:

Atque

ratus ipse suspectos non habuit, nobis imperaverit: obtinere jus belli et pacis inter bestias sui generis; cum tamen

Cornix cornici nunquam perfodit ocellum.

nec lupus lupum est; et quem in finem *mancipia* Marmotis pro victu colligendo necessaria? Quod certo certius novimus, Mures Alpinos vel per *octo menses* dormire, nullis obsoniis opus habentes; nec ullum mancipiorum negotium, quasi per tractatus *d'Assiento,* inter illos et alias bestias intercedere potest, nisi forte ipsis abuterentur, prout homines mancipiis auro solido comparatis jure utuntur ad fodiendam terram.

Quicquid autem sit juxta novellam historiam; juxta *antiquam fabulam* ridiculum audit; *equis ad posteriorem currus partem uti,* et inverso ordine naturæ, contra naturalem, pilorum et exuviarum dispositionem, mordicus prehensa, temonis loco, cauda animalis, idem animal, quasi maleficum et damnatum, veluti ad currum vel traham religatum trahere, inque ipsa via vecturæ adferre obstacula. Si pro lecto opus habent mures fœno, sufficit ejus parca collectio et transportatio vel ore vel pedibus anterioribus mediantibus, quod et bipedes ingredi posse bestiolas novimus.

Sic est: *Historia naturalis* non bene digesta abit in fabulam; præjudicia vero et nimia credulitas *Veritatem,* etsi cominus satis cognitam, longissime aliquando propellunt. Hoc est.

X.

Atque ubi tempestas bruma veniente rigescit,
Et complenda manet secto cellaria fœno,
Protinus ad messem ducunt servata ferendam
Mancipia, inversisque solum premere atque supinis
Corporibus, tum crura jubent attollere sursum,
Quatuor erectis perstent ut gramina palis.
Inde onerant caudaque trahunt animantia plaustra:
Erasoque vias miserorum tergore verrunt.

X. *Part of a Letter from Abbè* Nollet, *of the* Royal Academy *of* Sciences *at* Paris, *and F. R. S. to* Martin Folkes *Esq*; Presisident *of the same, concerning* Electricity.

Translated from the French, *by* T. Stack, *M. D. F. R. S.*

SIR,

Read Feb. 11. 1747-8. FOR several Years past Electricity has been my chief Occupation. Last Summer I read three Memoirs at our weekly Meetings, which contained many Particulars on this Subject: But as these were Matters of mere Curiosity, and of no real Use, they almost tired out my Patience. I now send you some Experiments, which I made during the Vacation, which seem to promise at least the being of some Service; but of this you will be the best Judge. I will describe them in the same Order as I made them, and to which I was not led by mere Accident. You know, that when a Vessel full of Liquor, which runs out through a Pipe, is electrified, the electrified Jet or Stream is thrown farther than usual, and is diverged into several divergent Rays, much in the same manner as the Water poured out from a watering Pot. Every body at first Sight will judge, that the Stream is accelerated, and that the electrified Vessel will soon be empty. I was unwilling to rely on the first Appearances, and therefore resolved to ascertain the Fact, by measuring the Time, and the Quantity of the Liquor running out.

 And

And in order to know if the Acceleration, supposing there was any, was uniform, during the whole Time of the running out, I made use of Vessels of different Capacities, terminating in Pipes of different Bores, from three Lines Diameter to the smallest Capillaries: And I give you in gross the Result of upwards of an hundred Experiments, as it is not so easy a Task to draw a safe Conclusion, as may at first be imagined.

1. The electrified Stream, tho' it divides, and carries the Liquid farther, is neither accelerated nor retarded sensibly, when the Pipe, thro' which it issues, is not less than a Line in Diameter.
2. Under this Diameter, if the Tube is wide enough to let the Liquid run in a continued Stream; the Electricity accelerates it a little, but less than a Person would believe, if he judged by the Number of Jets that are formed, and by the Distance to which it shoots.
3. If the Tube is a capillary one, from which the Water ought naturally to flow, but only Drop by Drop, the electrified Jet not only becomes continued and divided into several, but is also considerably accelerated; and the smaller the capillary Tube is, the greater in proportion is this Acceleration.
4. And so great is the Effect of the electrical Virtue, that it drives the Liquid out of a very small capillary Tube, thro' which it had not before the Force to pass, and enables it to run out in Cases, where there would not otherwise have been any Discharge.

These

These last Facts have served as a Basis to my Inquiries. I consider'd all organized Bodies as Assemblages of capillary Tubes, filled with a Fluid that tends to run thro' them, and often to issue out of them. In consequence of this Idea, I imagined, that the electrical Virtue might possibly communicate some Motion to the Sap of Vegetables, and also augment the insensible Perspiration of Animals. I began, by some Experiments; the Result of which confirm'd my Notions. I electrified, for four or five Hours together, Fruits, green Plants, and Sponges dipp'd in Water, which I had carefully weigh'd; and I found, that, after this Experiment, all these Bodies were remarkably lighter than others of the same kind, weigh'd with them, both before and after the Experiment, and kept in the same Place and Temper. I also electrified Liquors of all sorts in open Vessels; and I remarked, that the Electrification augmented their Evaporation, in some more, in others less, according to their different Natures. Wherefore I took two Garden-Pots, filled with the same Earth, and sowed with the same Seeds; I kept them constantly in the same Place, and took the same Care of them, except that one of the two was electrified for fifteen Days running, for two or three, and sometimes four Hours a Day. This Pot always shewed its Seeds raised two or three Days sooner than the other, a greater Number of Shoots, and those longer, in a given Time: Which makes me believe, that the electrical Virtue helps to open and display the Germs, and facilitates the Growth of Plants. I advance this, however, only as a Con-

 jecture,

jecture, which deserves further Confirmation; as the Season was already too far advanced, to allow me to make as many Experiments as I could have wish'd: But here are yet other Facts, of which I have a greater Certainty, and which are not less interesting.

I chose several Pairs of Animals of different kinds, Cats, Pigeons, Chaffinches, Sparrows, *&c.* I put them all into separate wooden Cages, and then weighed them. I electrified one of each Pair for five or six Hours together: Then I weighed them again. The Cat was commonly 65 or 70 Grains lighter than the other; the Pigeon from 35 to 38 Grains; the Chaffiinch and Sparrow 6 or 7 Grains: And in order to have nothing to charge upon the Difference that might arise from the Temperament of the Individual, I again repeated the same Experiments, by electrifying that Animal of each Pair, which had not been electrified before; and notwithstanding some small Varieties which happen'd, the electrified Animal was constantly lighter than the other in proportion.

Electricity therefore increases the insensible Perspiration of Animals: But in what Proportion? In the *Ratio* of their Bulks, or in that of their Surfaces? Neither of the one or the other, strictly speaking, but in a *Ratio* much more approaching to the latter than to the former. So that there is no Room to apprehend that a human Person electrified would lose near a 50th Part of his Weight, as it appear'd to me that it happen'd to one sort of Bird; nor the 140th Part, as to the Pigeon, *&c.* All that I have been hitherto able to learn upon this Head,

is,

is, that a young Man or Woman, from 20 to 30, being electrified during five Hours, lost several Ounces of their Weight, more than they were wont to lose, when they were not electrified. These last Experiments are difficult to pursue with Exactness; because the Cloathing, which cannot strictly be compared to the Hair or Feathers of Animals, retains a good Share of the perspired Matter, and hinders one from forming a good Judgment of the whole Effect of the electrical Virtue.

This forced electric Perspiration is very naturally accounted for, if we consider, that the electrical Matter pervades the interior Parts of Bodies, and that it visibly darts from within outward: For it is very plain, that these electrical Emanations must carry with them whatever they find in the small Vessels, thro' which they are seen, or at least are known, to issue.

This Explanation will, in my Opinion, occur to every one, who has seen the principal *Phænomena* of Electricity. But how shall we account for all the following Effects? All those Animals, whose Perspiration is increased upon their being electrified, all those Seeds, which shoot and grow quicker; all those Liquors, which evaporate; all that Acceleration of Liquids flowing thro' Tubes; all those Particulars, I say, happen in the same manner, when, instead of electrifying those Bodies themselves, they are only held near electrical Bodies of a pretty large Bulk. The Notion which I have, for these three Years past, formed of Electricity, not only affords me an Explication of this, as simple as the former, but I venture to say, it was this same Notion, that led

led me to the Experiments, and made me even foresee their Succeſs.

I am not only ſatisfied of the Exiſtence of an *effluent* electric Matter, which all the World allows, and which ſhews itſelf a thouſand Ways; but many convincing Reaſons have alſo aſſured me, that there is, round every electrified Body, an *affluent* Matter, which comes to it not only from the ambient Air, but likewiſe from all the other Bodies, whether ſolid or fluid, that are round about, and within a certain Diſtance of it. If theſe ſurrounding Bodies are of a ſimple Nature, as a Stone, a Piece of Iron, *&c.* nothing iſſues from them but pure electrical Matter: But if they are Animals, Plants, or Fruits, or, in a Word, any organized Bodies, or ſuch, in the Pores of which there is any Subſtance capable of giving way to the Impulſes of the electric Matter; this Matter will, in iſſuing forth with the great Rapidity, which it is known to have, carry along with it whatever it finds moveable enough to be diſplaced by it; and by ſo much will the Weight of the Body be diminiſhed; the ſame Effect being here produced by the *affluent* Matter, as is produced on electrified Bodies by the *effluent*. If you will pleaſe to read over my Eſſay, what I advance will be better underſtood. The Increaſe or Diminution of Perſpiration is not a Matter of Indifference to the animal Oeconomy: This new Method of increaſing it at Will may poſſibly prove of Uſe; it is neither inconvenient nor dangerous; and neither I myſelf, nor any body elſe of thoſe on whom I made my Experiments, ſuffered even the leaſt Inconveniency from it. One feels neither Motion nor Heat differing from that of the natural

State.

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State. Nor did the Animals give any Signs of Uneasiness, while they were electrifying: A little Weariness, and a better Appetite, were the only Effects we ever perceived.

As to the Facility of applying this Method, 'tis well known that the electrical Virtue is easily transmitted a good way off by Chains, &c.; and one may easily imagine, that an easy Chair, or even a Bed, suspended or supported in a proper manner, will put the most infirm Persons in a Situation to be very commodiously electrified. But as there is no Necessity to electrify them actually, it will become easier still; for nothing more will be requisite, than to place near them a Basket of old Iron render'd electrical. The commonest Degree of Sagacity will suffice to put this Method in Practice, whenever it is found to be useful.

I shall observe further, that, when I electrify an Animal, I render his Perspiration more copious; and this Effect is universal thro' every Part of it. When I only place it near an electrified Body, it perspires as much. But is its whole Body equally sensible of this Effect? I mean, what exhales in consequence of the Electricity, does it issue from every Part of his Surface? I believe it does not; and that for these Reasons.

If it be the electrical Matter of the Skin that drives out the Matter of Perspiration, by rushing towards the electrified Body, it is natural to think, that this Effect takes place only in the Part out of which the electrical Matter issues: Thus the Perspiration, which is electrically forced out, ought to issue from those Parts only, which are the most directly applied toward the electrical Body. Let us confirm this by Experiments.

To

To an electrified Body I apply a Veſſel full of Liquor, which iſſues Drop by Drop thro' ſeveral little Tubes placed in different Parts of its Circumference: Theſe Drops become continued Streams, and are accelerated, as if the Veſſel had been electrified: But this Effect is obſervable on that Side only which faces the electrified Body.

I moiſten a thick Sponge with Water, and cut it in two: I weigh theſe two Halves ſeparately; I join them again, and place the whole near a large electrified Body, ſo as to make one Half of the Sponge face the Body directly, and the other the contrary Way. After an Electrification of five or ſix Hours, that Half, which faced the electric Body, was found to be lighter than the other, *&c.*

Wherefore I think I have good Grounds to believe, that a Man, who preſents a Shoulder, or one Side of his Head, to a large electrified Body, perſpires more thro' that Part than thro' any other. Add to this, that ſince theſe Animals, which I cauſed to perſpire in this laſt manner, and which had but one Side of their Bodies expoſed to the Electricity, loſt as much of their Weight, as the others which were throughly electrified; it follows, that they perſpired as plentifully thro' the expoſed Part, as the others thro' the whole Body. Whence we may infer, that, of the two Methods, which I propoſe for augmenting inſenſible Perſpiration, the latter is the moſt powerful, and moſt proper to remove Obſtructions from the Pores, or to ſcour them of any noxious Humours which they may happen to contain. I have the Honour to be, with the greateſt Reſpect, Sir,

Your moſt humble and moſt obedient Servant,

The Abbè Nollet.

XI.

XI. *Several* Essays *towards discovering the* Laws *of* Electricity, *communicated to the* Royal Society *by Mr.* John Ellicott *F.R.S. and read on the 25th of* Feb. 1747. *and at two Meetings soon after.*

1. *A Letter to* Martin Folkes *Esq; Pr. R. S.*

SIR,

Read Feb. 25. 1747-8.

IN the * Letter lately read before the *Royal Society* from the Abbè *Nolet*, containing his Observations on the Increase of the Transpiration of Animals, and the Growth of Vegetables, by means of the electrical *Effluvia*, he takes notice, that he was led to those Inquiries, from the Acceleration which (he found from a great Number of Experiments) was given to the Motion of Fluids thro' capillary Tubes, upon their being electrified. As I formerly made several Experiments on this Subject, I shall submit it to your Consideration, whether the following Observations on those Experiments may deserve the Notice of this illustrious *Society*. In which I have principally endeavoured to prove, that the Acceleration of the Motion of Fluids thro' capillary Tubes or Syphons is not barely owing to their being electrified, but that, in all Cases whatsoever, there are some other Circumstances necessary, in order to produce this Effect. And I doubt not but to make this fully appear, by shewing, that Water, being electrified, may either be made to run

* See the preceding Paper, *Art.* X. *p.* 187.

in a conſtant Stream thro' a capillary Tube or Siphon, or only to drop, as if it had not been electrified at all: And likewiſe, that the Water may be made to run from the ſame Syphon in a conſtant Stream, without being made electrical, but ceaſe to run, and only drop, the Moment it becomes electrical. Under the one or other of theſe Caſes, I ſhall have an Opportunity of taking notice of the ſeveral Varieties obſervable in theſe Experiments; all of which I ſhall endeavour to account for from the following general Principles.

First, That the ſeveral electrical *Phænomena* are produced by means of *Effluvia.*

Secondly, That the Particles compoſing theſe *Effluvia* ſtrongly repel each other.

Thirdly, That the ſaid Particles are ſtrongly attracted by moſt if not all other Bodies whatſoever.

That the electrical *Phænomena* are produced by means of *Effluvia*, is in general acknowledged by all the Authors who have wrote upon Electricity, however they may differ in Opinion with regard to the Bodies in which they are contained. The Properties I have mention'd of theſe *Effluvia* may be eaſily deduced from moſt of the Treatiſes lately publiſhed on this Subject. But to leave no Room for any Objection, I would beg Leave to obſerve, that the Exiſtence of theſe *Effluvia* is proved by all thoſe Experiments in which a Stream of Light is ſeen to iſſue from the electrified Body; particularly thoſe Streams which are ſeen to iſſue in diverging Rays from the End of the original Conductor, when made of Metal, and reduced to a Point; from their

their being felt to strike against the Hand like a Blast of Wind, when it is brought near the Stream, and from that offensive Smell which generally accompanies these Experiments, and which is always more perceptible, the more strongly the Sphere is excited.

That the Particles composing these *Effluvia* repel each other, appears from those Experiments, in which two Bodies, how different soever they may be in kind, repel each other when they are sufficiently impregnated with these *Effluvia*. As a Feather, by the excited Tube; the several Fibres of the same Feather, or two Cork Balls, which will be found strongly to repel each other, so long as they retain any considerable Quantity of these *Effluvia*. Which Property will always decrease, as the Quantity they contain diminishes.

That these *Effluvia* are strongly attracted by most if not all other Bodies, is so evident from almost all the electrical Experiments, as to make any particular Examples of it needless here; especially as I shall have Occasion to take notice of the strong Attraction between the electrical *Effluvia* and Water, in accounting for these Experiments. And the first, I would take notice of, I shall now proceed to state as follows.

Experiment I.

If a Vessel of Water is hung to the prime Conductor, having a Syphon in it of so small a Bore that the Water will be discharged from it only in Drops, on the Water's becoming electrical by means of the Machine, it will immediately run in a Stream,

 and

and continue to do ſo, till the Water is all diſcharged, provided the Sphere is continued in Motion.

That Water does not run in a conſtant Stream, but only in Drops, from a Syphon of a ſmall Bore, is doubtleſs owing to the ſame Cauſe by which it is ſuſtained above the Level in capillary Tubes. If therefore Water is made to run in a Stream barely by its being impregnated with the electrical *Effluvia*, it ſhould follow, that if one or more capillary Tubes be placed in a Veſſel of Water, that which is ſuſtained in them would either ſink down to a Level with the reſt of the Water, on its being made electrical, or at leaſt that it would not continue at the ſame Height as before; but if the Experiment is made, the Water will be found to continue exactly at the ſame Height, whether it is electrified or not.

Again, if the bare electrifying the Water was the Cauſe of its running in a Stream, it would continue to run in the ſame manner, ſo long as the Water continued electrical, which it will not do: For, on ſtopping the Motion of the Machine, the Stream will immediately ceaſe, and the Water will only drop from the Syphon, notwithſtanding its being ſtrongly impregnated with the electrical *Effluvia*. To account then for the Water's being made to run in a Stream in this Experiment, I would obſerve, that ſo long as the Machine is in Motion, there is a conſtant Succeſſion of the electric *Effluvia* excited, and which viſibly run off from the End of the prime Conductor in a Stream, and as they are in like manner carried off from all Bodies hung to it, thoſe *Effluvia* which run off from the End of the Syphon, being ſtrongly attracted by the Water, carry

carry ſo much of it along with them, as to make it run in a conſtant Stream.

That the Attraction between the Water and electric *Effluvia* is ſufficient to produce this Effect, might be proved by a Variety of Experiments; but I ſhall only obſerve, that to this Attraction it is owing that ſilk Lines and glaſs Tubes (which, from their imbibing ſo very ſmall a Quantity of theſe *Effluvia*, are generally made uſe of as Supports in many of the electrical Experiments) on only being wetted become ſtrong Conductors: And that if an excited Tube is held over a Veſſel of Water, the Water is found to imbibe a very conſiderable Quantity of this electric Matter; and, on the Approach of a Finger, or any other non-electric Body, the Water will be perceived to riſe towards it; and if the Finger is brought ſo near the Surface as to draw off the *Effluvia*, they will carry ſeveral Particles of the Water along with them towards the Finger, in a Direction directly contrary to that of Gravity; and therefore may well be ſuppoſed, when acting in the ſame Direction, to have an Influence ſufficient to produce a Stream, as in the Experiment.

And that this Current of the electric *Effluvia* is the true Cauſe why the Water runs in a Stream from the End of the Syphon, is farther evident, in that whatever tends to increaſe or diminiſh the Current of the *Effluvia*, produces the ſame Effect upon the Water. I have already obſerved, that when the *Effluvia* are ſtrongly excited, they will be ſeen to paſs off from the End of the prime Conductor in luminous Rays; and the ſame may be obſerved with reſpect to thoſe which paſs with the

Water from the End of the Syphon; but if any non-electric Body is brought under the Syphon, as, by its Attraction, the Current of the *Effluvia* will be increased, so these luminous Rays will likewise extend to a greater Length. Again, if the Motion of the Machine is stopped, the Current of the electric *Effluvia* will thereby be stopped, and the Water will immediately cease to run in a Stream, notwithstanding its being strongly impregnated with the electrical *Effluvia*.

And that the Water is strongly impregnated will not only appear from the Drops being sooner divided into small Particles than they would be if they had not been electrified, but from those Particles being separated to a greater Distance from each other, by the repulsive Property of the electric *Effluvia*; and if any of the Water is received into a dry glass Vessel, on the Approach of a Finger towards its Surface, there will be seen a Spark to issue from it in the same manner as from Water electrified by an excited Tube; or if any non-electrical Body is brought under the Syphon, by whose Attraction the *Effluvia* may be drawn off, the Water will immediately be found to accompany it in a Stream.

Exp. II.

If the Vessel of Water with the Syphon in it is suspended by any non-electric Body over another strongly electrified, the Water will immediately run from the Syphon in a Stream; but if supported by a Piece of Silk, or any other electrical Body, the Water will immediately cease running, and only be

discharged

difcharged in Drops. Thefe *Phænomena* may, from what has been already faid under the former Experiment, be eafily accounted for.

That the Water is made to run in a Stream, is plainly owing to the mutual Attraction between the electrifed Body and the Water; which Attraction will continue, fo long as the Veffel which contains the Water, by being fupported by a Non-electric, is prevented from retaining any of the electrical *Effluvia*; thefe *Effluvia* being drawn off by the non-electric Body, to which the Veffel is fufpended: But on the contrary, when the Veffel is fufpended by an Original-Electric, the *Effluvia*, not being attracted thereby, will be prevented from running off, and the Water will foon be found to have imbibed a Quantity of them, fufficient, by their repelling Property, to greatly weaken, or wholly to deftroy, the former Attraction, when the Water will ceafe to run in a Stream, and only drop, as if it had not been held near any electrifed Body. Monf. *L'Abbe Nolet* has endeavoured to account for the former Part of this Experiment, by fuppofing there is, what he calls, both an affluent and an effluent electric Matter; but he takes no notice of the latter Part, which is not eafily folved upon his Suppofition. But if what I have obferved on thefe Experiments is fatisfactory, I apprehend I have accounted for the feveral *Phænomena* on much more folid Principles, and that thereby any lefs certain Hypothefis is render'd ufelefs.

I intended to have taken fome notice of the different Acceleration of the Fluids thro' Tubes of different Bores; but as this Acceleration will always

vary

vary with the Current of the electrical *Effluvia*, unless some Method could be found out to render this Current uniform throughout the whole Series of Experiments, the Profecution of this Inquiry will be rendered extremely difficult, and the Refult will at beft be very uncertain. I am Sir, with the greateft Refpect,

Your moft obedient humble Servant,

John Ellicott.

When the foregoing curious Letter was read at the Meeting of the *Royal Society* on *Thurfday* the 25th of *February* laft, 1747. I acquainted the Gentlemen prefent, that the fame ingenious Author had communicated to me a Paper feveral Months before, in which he had more fully and particularly delivered his Thoughts on the furprizing *Phænomena* of Electricity, and as feveral Perfons expreffed their Defire of feeing that Paper, I requefted of him either a Copy, or an Abftract of the fame; in Compliance with which he, fome Days after, gave me the two following Papers, containing the Subftance of what he had before fhewn me; and I immediately put them into the Hands of Dr. *Mortimer*, one of the Secretaries of the *Society*, who read them at the two Meetings of the *Society*, on the feveral Days noted at the Head of thofe Papers.

M. Folkes.

2. *An* Eſſay *towards diſcovering the* Laws *of* Electricity, *addreſſed to the* Royal Society.

Read March 24. 1747-8.

THE great Difference I obſerved in the Sentiments of thoſe ingenious Gentlemen who have favoured us with their Diſcoveries in Electricity, made me very deſirous of finding out ſome general Principles, by means of which I might be able to form a Judgment of the ſeveral Hypotheſes whereby they have endeavoured to account for the principal *Phænomena* obſervable in thoſe Experiments. In order to this I took a general Survey of all the more remarkable Experiments, and out of them made Choice of ſuch as I judged were moſt proper for my Purpoſe; and from theſe I deduced the general Principles hereafter mentioned. The Advantage I promiſed myſelf from this Method was, that the plainer and more ſimple the Experiments were, which I made choice of, the leſs liable I ſhould be to miſtake in any Concluſions drawn from them; and that every freſh Experiment, I could account for by them, would be an additional Proof in their Favour; and if my Attempt in explaining the following Experiments from thoſe Principles ſhould prove ſatisfactory, the Truth of them would be thereby ſo fully confirmed, that we might ſafely rely on them in forming a Judgment of any of the Diſcoveries already made; and (how general ſoever they may ſeem to be) I doubt not but they will be found of Service in proſecuting our future Inquiries on this Subject.

The Experiments from which I deduced theſe Principles were theſe which follow.

Experiment I.

If a glaſs Tube is rubbed by a very dry Hand, and a Finger is brought near any Part of it, a Spark of Fire will ſeem to iſſue from it, and ſtrike againſt the Finger; and if the Finger is carried at a like Diſtance from the End of the Tube towards the Hand in which it is held, a Number of Sparks at a ſmall Diſtance from each other will be ſeen coming from it, and a ſnapping Noiſe will be heard. The Tube is then ſaid to be excited, or to be electrical; and at ſome times, when it is ſtrongly excited, Sparks will iſſue from the Tube in Streams, not only while it is rubbing, but will continue to dart out from it for a conſiderable time after the Rubbing has ceaſed, and a very ſtrong offenſive Smell will be perceived.

Exp. II.

If the Tube, when thus excited, is held over ſome Pieces of Leaf-Gold, or any light Bodies whatſoever, they will be attracted towards it; and the more ſtrongly the Tube is excited, the greater Diſtance they will be attracted from; and when they come near the Tube (tho' without touching it) they will be repelled from it, and continue to be ſo, unleſs touched by ſome other Body, when they will be attracted by the Tube as before: But if the Tube is but weakly excited, they will be attracted quite to the Tube, to which they will ſometimes adhere, without being repell'd from it.

Exp. III.

If a Ball (of Cork ſuppoſe for Lightneſs) be hung by a ſilk Line, and the excited Tube is applied to it, it will not only be attracted, but will have an attractive Quality communicated to it from the Tube; and if any light Bodies are brought near the Ball, they will be attracted by it.

Exp. IV.

As the Tube, when ſtrongly excited, will not only attract, but afterwards repel any light Bodies brought near it, in like manner the Cork-Ball will be endued with the ſame Property; ſo that a ſmaller Ball will firſt be attracted towards it, and then repelled from it, the ſame as the Leaf Gold in *Exp.* 2. and on touching any other Body it will be again attracted; and this may be repeated ſeveral times, provided the ſmaller Ball is much leſs than the larger one, tho' the Effect will conſtantly grow weaker and weaker, as every time the leſſer Ball is attracted, it carries off with it ſome of the electric Virtue, and is likewiſe endued with the ſame Properties as the larger Ball.

Mr. *Gray*, Mr. *Dufay*, and others have obſerved, that this electrical Quality is not only to be excited in Glaſs, but in moſt ſolid Bodies capable of Friction (Metals excepted); tho' in ſome it will be ſcarcely ſenſible, and that it is found to be ſtrongeſt in Wax, Reſins, Gums, and Glaſs: And as Glaſs is the eaſieſt procured of a proper Form, it has generally been uſed in making theſe Experiments. It has been fur-

ther obſerved, that thoſe Bodies in which the electrical Quality is capable of being excited the ſtrongeſt by Friction will receive the leaſt Quantity of it from any other excited Body, and therefore are properly made uſe of to ſupport any Body deſign'd to receive the electrical Virtue. The Truth of this will ſufficiently appear from the following Experiment.

Exp. V.

Hang up two Lines, one of Silk, and the other of Thread; that of Thread will be attracted by the Tube at a much greater Diſtance than the Silk. Again; Faſten to each String a Feather, or other light Body; if the Tube is brought to the Feather faſtened to the Silk, it will be firſt attracted, and afterwards repelled; and from the Virtue communicated to it from the Tube, the ſeveral Fibres of the Feather will ſtrongly repel each other. But when the Tube is brought to the Feather faſtened to the Thread, the Feather will be ſtrongly attracted, and continue to be ſo, without ever being repell'd, the Virtue paſſing off by the Thread it is hung to. If a glaſs Ball is hung to the ſilk Line, it will be but weakly attracted by the Tube; but one of Cork or Metal much ſtronger.

Exp. VI.

Let a Rod of Iron be ſuſtained by ſilk Lines, and by means of a glaſs Sphere (which can be more regularly and conſtantly excited than a Tube) be made electrical; it will be found to have all the Properties of the excited Tube mention'd in *Exp.* 1. A

Stream

Stream of Light will come from the End of it, if it is pointed; it will attract, repel, and communicate this Virtue to any other non electric Body: On the Approach of a Non-electric, a Spark of Fire, with a Snap attending it, will come from it; which Spark will be greater or less, as the Bodies approaching it have more or less of the electrical Quality residing in them; and there will likewise be the same offensive Smell as was observed of the Tube.

From these Experiments, which I think contain the principal *Phænomena* of Electricity, may justly be drawn the following Conclusions:

1*st*. That these remarkable *Phænomena* are produced by means of *Effluvia*; which, in exciting the electrical Body, are put into Motion, and separated from it.

2*dly*. That the Particles composing these *Effluvia* strongly repel each other.

3*dly*. That there is a mutual Attraction between these Particles, and all other Bodies whatsoever.

That there are *Effluvia* emitted from the Tube when rubbed, and which surround it as an Atmosphere, is evident, from that offensive Smell arising from them, from that Sensation on the Hands or Face, when the Tube is brought near either of them, and from those Sparks of Light, on a still nearer Approach of the Finger to it.

That the Particles of these *Effluvia* repel each other, is proved by the Cork-Balls (*Exp.* 4.) and the Fibres of the Feather (*Exp.* 5.) repelling each other, when impregnated with them; and by the Leaf-Gold (in *Exp.* 2.) being repelled by the Tube, and

not

not returning to it again, until, by coming near, or touching, some non-electric Body, the *Effluvia* are drawn off from it.

From this Property it is, that these *Effluvia* expand themselves with so great a Velocity whenever they are separated from the electric Body; and as they are likewise capable of being greatly condensed, may we not from hence justly conclude they are elastic?

That there is a mutual Attraction between these *Effluvia* and most other Bodies, appears from their collecting from the Tube such Quantities thereof, as to endue them with the same Properties with the Tube itself, as was proved by the *3d*, *4th*, and *5th* but more particularly by the *6th Experiment*.

These Principles being admitted, it will follow, that the greater Difference there is in the Quantity of electrical *Effluvia* in any two Bodies, the stronger will be their Attraction. For, if the *Effluvia* in each are equal, instead of attracting, they will repel each other; and in proportion as the Quantity of electric Matter is drawn from one of the Bodies, will the Attraction between them increase, and consequently be strongest, when any one of them has all the electrical Matter drawn from it.

The Particles of these *Effluvia* are so exceeding small, as easily to pervade the Pores of Glass, as is evident, in that a Feather, or any light Bodies inclosed in a glass Ball hermetically sealed, will be put in Motion on the excited Tube being brought near the Outside of it; and it has been generally thought that they pass through the Pores of the densest Bodies; and there are several Experiments

which

which render this Suppoſition not improbable; tho' I muſt acknowledge I have not yet met with any one that I think is quite concluſive.

I ſhall now proceed to ſhew, how, from theſe Principles, the *Phænomena* of ſome of the more remarkable Experiments of Electricity may be accounted for.

Exp. VII.

Let a Rod of Iron, pointed at one End, be ſuſpended on ſilk Lines, as in *Exp.* the *6th*, and by the Sphere be made electrical. When the Rod is ſtrongly electrified, a Stream of Light in diverging Rays will be ſeen to iſſue from its Point; and if any non-electric Body is held a few Inches from the Point, the Light will become viſible to a greater Diſtance, and if the non-electric Body is likewiſe pointed, a Light will ſeem to iſſue from that in diverging Rays in the ſame manner as from the electrified Rod. But if the non electrical Body is flat, and held at the ſame Diſtance from the Rod as the pointed one was, no Light will be ſeen to come from it.

The principal *Phænomena* to be accounted for in this Experiment are; Why a Light is only ſeen at the Point of the Rod, and not through the whole Length of it? Why this Light is viſible to a greater Length, when the Point is approached by a Non-electric? And, Why a Light is ſeen to iſſue from the Non-electric when it is pointed, and not when it is flat?

Upon which I obſerve, that whenever the Sphere is excited, the electrical *Effluvia* are thereby put into Motion, and made to form an Atmoſphere

round about it, from whence, by their repulſive Property, they endeavour to expand themſelves on all Sides equally; but being ſtrongly attracted by the Iron, a great Part of them are drawn off along the Rod, about whoſe Surface they likewiſe form an Atmoſphere, which will be denſer or rarer, in proportion as the Attraction of the Rod is greater or leſs; and as the repulſive Power of theſe *Effluvia* will always increaſe in proportion with their Denſity, it will follow, that whenever the Sphere is ſo ſtrongly excited, that the *Effluvia* ſurrounding it are denſer than thoſe ſurrounding the Rod, they will, by their repulſive Property, drive the *Effluvia* off from the End of it in a Stream, and that with a very great Velocity; as is evident, from their ſtrikeing againſt the Hand like a Blaſt of Wind when brought near the End of the Rod: And as this Velocity is partly owing to the Attraction of the Rod, ſo this Attraction continuing quite to the End of it, the Velocity of the Particles will there be greateſt; and as they approach towards the Point, they will be brought nearer together, and therefore become denſer there than in any other Part of the Rod; and therefore if the Light is owing to the Denſity and Velocity of the *Effluvia*, it will be viſible at the Point, and no-where elſe.

And that the Light is thus produced, will appear, in that whatever increaſes or diminiſhes either the Velocity or Denſity of the Particles will increaſe or diminiſh the Light. For, let the Motion of the Wheel which turns the Sphere be ſtopped, the Current of the *Effluvia* will likewiſe be ſtopped, and the Rays of Light will no longer be ſeen to iſſue from the Point,

Point, and yet the whole Rod will continue to be electrical; but, on putting the Sphere again into Motion, the *Effluvia* will become visible as before, and will increase, as the Sphere is more strongly excited. Again, the Light will be visible to a greater or less Distance, as the Point is more or less acute; and as this Light is always brightest next the Point, and grows fainter, as the Rays diverge, this is plainly owing to the different Density of the Rays at equal Distances; for, when the Point is more acute, the Rays will diverge less, and therefore will be denser to a greater Distance than when it is less acute.

When a Non electric, whose End is flat, is brought within a few Inches of the Point of the electrified Rod, the electric Stream will be attracted by it, and the Rays made to diverge less than before; and the Effect will be the same as if the Point was more acute; *viz.* a Continuation of the Light to a greater Distance, and which will be farther increased by the additional Velocity the Particles will acquire from the Attraction of the Non-electric. What will follow on a nearer Approach of the Non-electric to the Rod, will be consider'd under the next Experiment.

If the Non-electric is pointed, and held in the same Place as the former, a Light will appear from it the same as from the electrical Body: For, as the Points of the two Rods are the Parts which approach nearest each other, the Attraction there will be strongest: The Rays therefore, which diverged from the electrical Rod, will be attracted by, and made to converge towards, the Point of the non-electrical Rod, and will consequently be nearly of the same Density at the one as the other; and the

Velocity being accelerated by the additional Attraction, the Rays will become luminous at the Point of the Non-electric, the ſame as at the Point of the electrified Rod. If this Experiment is made with a Tube, inſtead of a Sphere, as it cannot be ſo uniformly excited as the Sphere, the Light will iſſue from the Rod in Flaſhes, as the Tube is more or leſs excited.

Several very ingenious Gentlemen, and in particular the Abbé *Nollet* have imagined, that the Light ſeen at the Point of the Non-electric was produced by means of *Effluvia* iſſuing from it in diverging Rays towards the electrified Rod, and which Current of *Effluvia* is therefore ſuppoſed to be the Cauſe of the attractive, as a like Current iſſuing at the ſame time from the electrified Rod is ſuppoſed to be the Cauſe of the repulſive Property of Electricity.

This Conjecture being directly contrary to the Account I have given of this *Phænomenon*, I ſhall offer ſome Conſiderations in Support of what I have advanced, and which I think will make it appear highly improbable, that any ſuch Current of *Effluvia* iſſues out of the Non-electric; but as what I have to offer on this Subject would treſpaſs too much on the *Society*'s Time at preſent, I ſhall defer it to my next Paper. I am,

Gentlemen,

Your obliged humble Servant,

John Ellicott.

3

3. *A Continuation of the foregoing* Essay.

Read May 19 1748. IN a Paper I had the Honour to communicate to this *Society*, *March* 24, I endeavour'd, from the Principles therein laid down, to account for some of the most remarkable *Phænomena* of Electricity; and in particular for that Appearance of a Light issuing from the End of an iron Rod, when pointed, and made electrical; Why this Light was visible only at the Point, and in no other Part of the Rod: Why the Light was visible to a greater Length when the Point was approached by a Non electric: And why a Light will be seen as issuing from the Non-electric when it is pointed, but not when it is flat.

I shall now endeavour, from the same Principles, to account for those *Phænomena*, which will be produced on a nearer Approach of the Non-electric to the electrified Rod.

Exp. VIII.

If the non-electric Body, whether flat or pointed, is brought nearer to the End of the Rod, than in the last Experiment, there will be a small Stream of Light produced, reaching quite from the electric to the non-electric Body; and if brought still nearer, there will issue a Spark attended with a small snapping Noise, which will be succeeded by others at equal Intervals; and if the Non-electric is held at some Distance from the Side of the Rod, the Point of it will frequently appear luminous, but no Part

 of

of the electrified Rod will be ſo. If it is brought nearer, there will likewiſe be Sparks produced at nearly equal Intervals from each other, which will ſometimes appear as iſſuing from the Side of the electrified Rod, at others, as coming from the Non-electric.

If a Finger is uſed as the Non-electric, it will receive a ſmart Stroke; and if Spirit of Wine, heated ſo as to emit an inflammable Vapour, is made uſe of, it will be kindled by the Spark.

Theſe *Phænomena* may, on the afore-mention'd Principles, be thus accounted for.

If the non-electric Rod is pointed, and brought ſo near, as, by its Attraction, to prevent the Rays iſſuing from the Point of the electrified Rod from diverging, they will be drawn off parallel to each other, and conſequently be equally luminous throughout the whole Diſtance between the two Rods.

If the Non-electric be brought ſtill nearer, the attractive Force will be ſo much increaſed, as not only to affect the *Effluvia*, when they are driven off from the Point of the electrified Rod, but to be capable of drawing them off from a conſiderable Part of the Rod beyond the Point; and that with a Velocity, and in a Quantity, ſufficient to occaſion both the Spark and Blow, as well as the Noiſe that is heard.

The ſame is the Caſe, when the non-electric Rod, or a Finger, is held againſt the Side of that which is made electrical: At a greater Diſtance a Light will appear as iſſuing from the Non-electric, the Particles attracted from a large Surface of the Rod (and therefore not viſible as coming from it) being made to converge to a Point, are thereby rendered

dered luminous, and, if brought nearer, there will issue Sparks in the same manner as when held to the End: And that this is owing to the Increase of the attractive Force, seems plain; for it was observed in the last Experiment, the Attraction was capable of changing the Direction of the Rays at the Distance of several Inches; whereas a Snap or Spark is seldom produced, when the Non-electric is held more than an Inch and half distant. If therefore the Attraction decreases, as the Squares of the Distances increases, as it probably does, the attractive Force will be many times greater in one Case than in the other, and if where the attractive Power was weaker, as in the former Experiment, there were so many Rays of the electric Matter collected, as to be sufficient to produce a Light, it cannot be thought extraordinary, when the Attraction is so greatly increased on the nearer Approach of the Non-electric, that both the Density and Velocity of the Particles should be thereby increased, so as to produce Heat sufficient to fire the Vapour arising from Spirit of Wine, or any other inflammable Vapour.

And that the Quantity of the electric Particles is greatly increased, as well as their Velocity, is evident from that large Surface of the Rod, which, by the Approach of a Finger, is in one Spark divested of them; and which requiring some time before it can be again sufficiently recruited, I apprehend is the Reason of that Interval between the Sparks. And here it must be observed, that the Distance the Point of the Non-electric is held at from the Rod, in order to produce the greatest Spark, must be varied, in proportion as the Rod is electrified in a greater or less Degree; the more strongly the Rod

is

is impregnated the greater will be the Diſtance; and if then the Non-electric is brought nearer, the Sparks will be ſmaller, but ſucceed each other quicker; ſo that when it is brought almoſt to touch the Rod, they will appear like a ſmall Stream. The Reaſon of which I take to be, that as the electric Atmoſphere ſurrounding the Rod is denſer nearer it than farther off, when the Non-electric is brought into ſo very denſe a Part of the Atmoſphere, it will from thence become nearly as electrical as the Rod itſelf; and therefore loſe great Part of its attractive Force, and conſequently will only be able to draw off thoſe Particles from the Rod which are neareſt to it.

I would farther take notice, that the Sparks are always produced in the Space between the Non-electric and the Rod, and often appear as iſſuing from the Non-electric. This Appearance is probably owing to thoſe Particles, which, by their Elaſticity, are reflected back again from the Non-electric towards the Rod, and which, by ſtriking againſt thoſe coming from it, produce both the Sparks and Noiſe that is heard; and as I have already ſhewn, that the Particles often appear in luminous Rays at the Point of the Non-electric, it thence happens, that the Spark is frequently kindled ſo near to the Non-electric, as to appear as iſſuing from it.

I obſerved, in my former Paper, that ſeveral ingenious Gentlemen, from this Appearance of a Light at the Point of the Non-electric, have imagined there was a Current of electrical *Effluvia* continually iſſuing out of it, and which, ſetting in towards the electrified Rod, was the Cauſe of the Attraction of the Electricity: And this Conjecture of theirs

will

will ſeem to be greatly favoured by the following Experiment.

If ſome of the Fibres of a Down-Feather be faſtened to the End of a ſmall Skewer or Wire, and made electrical, they will ſtrongly repel each other, and will expand themſelves on all Sides to the greateſt Diſtance poſſible from each other; but if a non electric Perſon bring the Point of a Pair of Compaſſes, or any other ſmall-pointed Body near them, they will be repell'd from it, and driven up together as with a Blaſt of Wind, and, in the dark, a Light will be ſeen as iſſuing from the Point; from whence it might be concluded, that the Fibres are repell'd by *Effluvia* iſſuing out of the Point of the Non-electric.

As the Abbé *Nollet* endeavours to account for the Attraction of Electricity on this Principle, I ſhall offer ſome Conſiderations, which, notwithſtanding theſe Appearances, have induced me to be of a different Opinion; and they are founded on the following Obſervations.

Firſt, That however replete any Bodies may be with the electric Matter, none of theſe *Phænomena* are ever produced, unleſs the *Effluvia* are firſt excited in ſome particular Body, and put in Motion, either by rubbing, or ſome ſuch-like Operation.

Secondly, That the *Effluvia* are not to be equally excited in all Bodies, but much ſtronger in ſome than in others; and that, in particular, they are not capable of being at all excited in Metals by Friction.

Thirdly,

Thirdly, The attractive and repulsive Property will be stronger or weaker in any Body, in proportion to the Quantity of excited *Effluvia* wherewith it is impregnated.

Fourthly, That those Bodies which are most easily excited by Friction, will receive the least Quantity of the electrical *Effluvia* from any other excited Body; and, on the contrary, Metals, or those Bodies in which they cannot be excited by Friction, will receive the most.

From these Observations I think it may be shewn, that this Appearance of Light is so far from proving that the *Effluvia* come out of the Non-electric, at whose Point they are visible; that from thence it cannot be concluded the Body has any of the electrical Matter residing in it, but is rather a Proof to the contrary. For I have already shewn, that the same Appearance would be produced from the setting in of the *Effluvia* into the Non-electric; and this might be confirmed, if necessary, by a Variety of Experiments. And as those Bodies, at whose Point this Light appears the strongest, afford us no Signs of their having any of the electrical *Effluvia* residing in them, either by their attracting or repelling other Bodies, or by their being capable of being excited in them by Friction, as in Glass, *&c.* nor in short any sort of Evidence whatsoever, but what arises from this Appearance; may we not expect some better Proof of their being possessed of these *Effluvia*, before we admit of their issuing out of them?

Again, it appears very extraordinary, that those Bodies, in which the *Effluvia* cannot be excited by

any

any other Method, ſhould ſend forth ſuch Streams of them, only on their being brought within a few Inches of the electrified Rod, and that theſe Streams ſhould increaſe as the Rod is more ſtrongly electrified; and yet that few or none of theſe Streams ſhould iſſue from thoſe Bodies in which the *Effluvia* can be excited: And if the firſt-mention'd Bodies are themſelves ſtrongly impregnated, the Streams will diſappear, and they will be ſo far from parting with any of their *Effluvia*, that, on the contrary, they will be ſtrongly repelled by the Rod.

I farther apprehend, on this Suppoſition, it will be extremely difficult, if not impoſſible, to account for the ceaſing of the Stream from the Point of the Non-electric on ſtopping the Machine; as likewiſe that the Rod ſhould ſo ſoon be diveſted of its *Effluvia*, on ſuch a Non-electric's being held near it, which it would otherwiſe retain for ſeveral Hours, and which I think is a ſtrong Proof of the *Effluvia*'s paſſing from the Rod into the Non electric. And that it certainly does ſo, may be confirmed by the Perſon who holds the Non-electric ſtepping upon a Cake of Wax, when he will ſoon become electrical, from the *Effluvia* he will receive (thro' the Point of the Non-electric) from the Rod; but ſo long as he continues to ſe ſo, there will not be ſeen any Light to iſſue from the Point; which I apprehend cannot be accounted for on any other Principle, but that of the ſetting in of the *Effluvia* at the Point of the Non-electric. And as I have already ſhewn, that all the *Phænomena* are naturally to be accounted for on this Principle, without being liable to any of the above-mention'd Objections, I muſt remain of the Opinion (till I can ſee

these Objections anſwer'd) that this Appearance of Light is no Proof that the *Effluvia* iſſue out of the Non-electric, but of the direct contrary.

The above-mention'd Objections might be brought, with equal Force, againſt the Fibres of the Feather being repelled by *Effluvia* iſſuing out of the Point of the Non-electric that is held near it, and in particular, that this Effect would ceaſe to be produced, either when the Machine was ſtopped, or the Perſon who held the Point became electrical. And to theſe I would add, that if this was really the Caſe, the Fibres would continue to be repell'd, notwithſtanding any Alteration in the Shape of the Non-electric; whereas, on the contrary, if the Joint of a Pair of Compaſſes was held towards them, inſtead of the Point, they would be ſtrongly attracted to it: And the ſame will always happen, whenever an obtuſe Body is brought near them inſtead of a pointed one.

The true Cauſe of this remarkable *Phænomenon* I apprehend to be the different Denſity of the *Effluvia* at the Extremities of the two Bodies; for I have already ſhewn the *Effluvia* will be much denſer at the Extremity of a pointed Body than at an obtuſe one: And as the Force by which the Particles endeavour to expand themſelves, increaſes in proportion to their Denſity, it follows, that the Particles will be reflected back with greater Violence from the pointed Body than the other; and this Force exceeding the attractive Power of that particular Part of the Feather, to which it is directed, the Fibres will be repelled by it; whereas the Force, with which the Particles endeavour to expand themſelves from the obtuſe Body, being leſs than the attractive Power,

it

it follows, that the Fibres of the Feather will continue to be attracted by it.

Exp. IX.

Take two Plates of Metal, very clean and dry, whose Surfaces are nearly equal; hang one of them horizontally to the electrified Rod, and bring under it upon the other any thin light Body, as Leaf-Silver, *&c.* when the upper Plate is made electrical, the Silver will be attracted by it; and if the under Plate is held at a proper Distance, will be perfectly suspended at right Angles to the Plates, without touching either of them; but if they are either brought nearer together, or carried farther asunder, the Leaf-Silver will cease to be suspended, and will jump up and down between them. The same Effect will be produced, if you reverse the Experiment, by electrifying the bottom Plate, and suspending the other over it.

If the upper Plate is electrified when the Leaf-Silver is brought near, it will be attracted upwards by it, and thereby become electrical; and so long as it continues to be electrical, it will likewise be attracted downwards by the non-electrical Plate. Whenever therefore this last Attraction added to the Gravity of the Silver, which acts in the same Direction, is equal to the contrary Attraction upwards, the Leaf-Silver will, by means of these two opposite Forces, be kept suspended between the Plates, and will continue to be so, as long as the Equality of these Forces is preserved.

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I have already ſhewn, that the Attraction between any two Bodies will always be in proportion to the different Quantity of electric *Effluvia* they are poſſeſſed of; the greater that Difference is, the greater will be the Attraction. In order therefore to obtain this equal Attraction at firſt, the Leaf-Silver muſt be imbued with a greater or leſſer Quantity, in proportion as the Plate is more ſtrongly or weakly electrified; but always with a much leſs Quantity than the Plate; and likewiſe the lower Plate will require to be placed at different Diſtances, in proportion to the Quantity of electric Matter the upper Plate is poſſeſſed of. As therefore the Suſpenſion of the Silver depends upon the exact Proportion of Attraction (ariſing from the different Quantities of electric Matter) in the two Plates and Leaf-Silver, it follows, that whatever alters the Quantity contained in any one of them would prevent the Suſpenſion.

It is well known, that, by the Attraction between any two Bodies, the electric *Effluvia* are continually drawn off from that which has the greateſt Quantity of them, till the other being ſufficiently impregnated, the Attraction ceaſes. In order therefore to preſerve theſe Proportions, it is neceſſary, that, as faſt as the non-electric Plate draws off any of the *Effluvia* from the Leaf-Silver, it ſhould part with it again; and ſo, by continuing to be a Non-electric, an equal Degree of Attraction be preſerved; and again, that the Leaf-Silver ſhould receive a freſh Supply from the electrical Plate, equal to what it conſtantly parts with; and the electrical Plate muſt likewiſe receive an equal Supply from the Globe;

and

and that there is ſuch a conſtant Current of the electrical *Effluvia*, is evident, from thoſe ſmall Streams of Light, viſible at the two Corners of the Silver next the Plates. If therefore the Globe ſhould be ſtopped, or the under Plate by any means become electrical, theſe Proportions would be thereby deſtroy'd, and the Leaf-Silver would ceaſe to be ſuſpended.

That the Leaf-Silver is always nearer to the non-electrical than to the electrified Plate, is owing to its receiving its Supply of *Effluvia* from the Atmoſphere ſurrounding the electrified Plate: For as the Plate is more ſtrongly electrified than the Silver, its Atmoſphere of *Effluvia* will be denſer to a greater Diſtance than that ſurrounding the Leaf-Silver, and therefore can ſupply an equal Quantity at a greater Diſtance than what the lower Plate can receive from the Silver, whoſe Atmoſphere is rarer; and therefore, as the Silver will always be ſuſpended in that Part where the two Currents are equal, without which I have already ſhewn the Proportion would be deſtroy'd, it will conſequently be always nearer to the non-electrical than to the electrified Plate. If the Experiment is reverſed, by electrifying the under Plate, and making the upper one the Non-electric, the only Difference will be, that the Gravity of the Silver muſt then be added to the Attraction of the electrified Plate, and will therefore cauſe the Silver either to be nearer the non-electrical one, or the Plates to be moved a little farther aſunder, or perhaps both.

I ſhall not at preſent preſume to take up any more of the Time of this illuſtrious *Society*; hoping that

that I have already ſhewn how the principal *Phænomena* of Electricity may be accounted for, upon the few Principles I have laid down; and however in different Experiments the Effects produced may either be varied, or increaſed, I doubt not but they may all be eaſily accounted for from the ſame Principles; as I ſhall willingly attempt to make appear at ſome more convenient Time, ſhould it be thought neceſſary. In the mean time I have the Pleaſure to ſubſcribe myſelf,

Gentlemen,

Your moſt obedient humble Servant,

John Ellicott.

XII. *A brief account of a Roman* Teſſera, *by Mr.* John Ward *F. R. S. & Prof. Rhetor. Greſh.*

Read March 3. 1747.

THE braſs plate, which accompanies this paper, and has been the occaſion of it, was dug up ſome time ſince at Market-ſtreet in Bedfordſhire; which lies in the Roman road called *Watlingſtreet*, about five miles on this ſide Dunſtable; and was brought to the *Society* by their worthy Member, *Samuel Clark* Eſq;.

The

The inscription ingraven on the two sides is,

TES. DEI. MAR
SEDIARVM

See the TAB.

Which Words may, as I apprehend, be read at length in the following manner:

Tessera Dei Martis Sediarum.

The first abbreviated word TES. I take to stand for *Tessera*, a *dye* or *cube* (*a*), so called from the Greek word τέσσαρα or τέσσερα, *four*; respect being had to its number of sides, distinct from the two horizontal planes, above and below. And under this consideration it was distinguished from the *Talus*, which being round at each end contained only four planes or faces, whereon it could stand, and therefore when thrown had no more than two side faces in view. Hence *ludere talis et tesseris* are spoken of by Roman writers as two different games (*b*).

But if this was the first and original notion of the word *Tessera*, it was applied afterwards to many other things; and that not so much from a similitude in the figure, as from the relation they bore to some other thing, of which they were the sign or token; as the points on the upper plane of the dye denoted the good or ill success of the cast. To recite the several uses of this word would both be tedious and unnecessary; and therefore I shall mention some few only, from which the design of this plate may the more easily appear.

And

(*a*) *Macrob. In somn. Scip. lib.* II. *cap.* 2. (*b*) *Cic. De senect. cap.* 16.

And I ſhall begin with the *Teſſera hoſpitalis*, which was either public or private. As to the former, we find among the inſcriptions publiſhed by *Gruter* inſtances of two municipal towns, who put themſelves under the patronage of a Roman governor. And the reciprocal ingagement between them, which was ingraved on two copper plates, in the form of an oblong ſquare, with a pediment at the top, is called in both TESSERA HOSPITALIS (*a*). The deſign of the latter was to cultivate and maintain a laſting freindſhip between private perſons, and their families; and gave a mutual claim to the contracting parties, and their deſcendants, of a reception and kind treatment at each others houſes, as occaſion offered. For which end it was requiſite, that thoſe *Teſſerae* ſhould be ſo contrived, as might beſt preſerve the memory of that tranſaction to poſterity (*b*). And one method of doing this was by dividing one of them lengthwiſe into two equal parts, upon each of which one of the parties wrote his name, and interchanged it with the other. A draught of one made of bone, and ſo divided, may be ſeen in *Thomaſinus*, with the name of the perſon on each part. Upon one of them is,

POLYNICES
ASCANIO. F.

And upon the other,

CLAPHYR
ANDRAE. M. I. F.

The

(*a*) *Pag.* CCCLXII, CCCLXIII.

(*b*) *Plaut. Pænul.* v. 1. 22. v. 2. 87.

The names are writen on the inside; and when the two parts were put together, they made a cylinder (*a*). From this custom came the proverbial expression, *tesseram hospitalem confringere*; which was applied to those persons, who violated their engagements (*b*).

The *tesserae frumentariae* are often mentioned by Roman writers, which were small tallies given by the emperors to the populace at Rome, intitling them to the reception of a certain quantity of corn from the public at stated seasons. And those, who were possessed of them, when they did not want the corn, sometimes sold them to others; as we learn from the satyrist, when he sais:

Summula ne pereat, qua vilis tessera venit
Frumenti (*c*).

The person, who had the inspection of these *tesserae*, and distributed the corn to those, who produced them, seems to have been called *tesserarius*; as *Pignorius* observes from a funeral monument, inscribed SYMPHORO TESSERARIO SER. CAESARIS (*d*). These *tesserae* were first made of wood, as appears from the words of *Pliny*, where treating upon the nature and properties of trees he sais, *Ligustra tesseris utilissima* (*e*). But *Fabretti* has published the draughts of two of them made of stone, in the form of cylinders, and of the same size with the originals. The length of them is somewhat short of three inches, the diameter three quarters of an inch, and the following

(*a*) *De tesser. hospital. cap.* 15. (*b*) *Plaut. Cistell.* II. I. 29.
(*c*) *Juvenal. Sat.* VII. 174. (*d*) *De servis, pag.* 318. *ed.* 1674, *oct.* (*e*) *H. N. Lib.* XVI. *cap.* 8. § 31.

lowing inſcriptions cut upon them contain the names of the perſons, to whom they belonged:

TORQVATVS CREOP. D. I. LVPVS PELORI D. I.

Where D. I. the two laſt letters in each inſcription ſtand, as he ſuppoſes, for *die prima menſis*, the time appointed for receiving the corn (*a*).

There was alſo another ſort of *teſſera*, not much unlike theſe, which intitled perſons to a ſight of the public games and other diverſions; but they are generally made in the form of an oblong ſquare. *Pignorius* has given us the draught of one in his own poſſeſſion, which conſiſted of ivory. Upon one of the ſides is the name PHILOMVSVS PERELI, on the next SPECTAVIT, on the third a *trident*, and upon the fourth a *palm branch* (*b*); the two laſt of which plainly ſhew, that it was given for admiſſion to the combats of the gladiators. Others of them had on different ſides the name of the perſon, with the day, on which the ſhow was exhibited, and the names of the conſuls at that time. Inſtances of theſe may be ſeen in *Thomaſinus*, one of which, as he ſais, was made of yellow glaſs (*c*).

But the *teſſera militaris* moſt frequently occurs in the Roman hiſtorians, which was the ſignal given by the general, or cheif commander of an army, as a direction to the ſoldiers for puting in execution any duty or ſervice required of them. This upon urgent occaſions was only vocal; as for a ſudden march,

(*a*) *Inſcript. antiq. pag.* 530. (*b*) *Ubi ſupra, pag.* 38.
(*c*) *De teſſ. hoſpital. cap.* 15.

march, or an attack upon the enemy. But in ordinary cafes, as for feting the watch, taking their diner, or the like, it was writen on a tablet. Tho in either way it was firft given by the general to the officers next in rank, and from them to the fubalterns, till it came to the perfon, whofe province it was to communicate it to the foldiers in each company. This tablet was commonly made of wood, as appears from *Polybius*, who calls it ξυλήφιον, *a fmall piece of wood* (*a*). The fignal infcribed upon it was very fhort, and ufually comprifed in one or two words; as *victoria*, *palma*, *virtus*, *Deus nobifcum*, *triumphus imperatoris*, mention'd by *Vegetius* (*b*); with many others of the like nature, which may be feen in antient writers. The perfon, whofe office it was to impart the fignal immediatly to the foldiers, is by *Vegetius* called *tefferarius* (*c*). Hence in *Gruter*'s infcriptions we meet with AVRE. IANVARIVS. TESSERARIVS. LEG. XIII, and C. GALERIO. C. LIB. AGATHON. TESSERARIO. COH. XII. PRAET. MILITVM, as alfo L. POMPEIO. L. F. POMP. REBVRRO. TESSERARIO. IN. CENTVRIA... (*d*). By which different forms of expreffion compared together one would be lead to conclude, that every century had its *tefferarius*, from whom the foldiers immediatly received the fignal; and that when the legion or cohort only is mentioned, the meaning is not, that the perfon named in the infcription performed that office

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(*a*) *Lib.* VI. *pag.* 479. *ed. Parif.* (*b*) *Lib.* III. *cap.* 5.
(*c*) *Lib.* II. *cap.* 7. (*d*) *Pag.* DCVI. 10. DCVIII. 7. DCIX. 10.

office to the whole legion or cohort, but only to ſome particular century in each of them.

But beſides theſe civil and military *teſſerae* there were others, which more eſpecially related to religious affairs, and may therefore be called ſacred; to which the inſcription on this braſs plate ſeems to agree. For the two next words ingraven upon it, namely DEI. MAR. muſt, I think, ſtand for *Dei Martis*. And if the laſt word SEDIARVM be taken for the name of a town, called *Sediae*, this *teſſera* may reſpect the *God Mars*, as the tutelar deity of that place. The religious worſhip among the Romans conſiſted cheifly in ſacrifices and other public ceremonies, the expenſe of which in particular places was ſupported either by the contributions of the inhabitants, or by private gifts. We have an inſtance of the latter in an inſcription firſt publiſhed by *Reineſius*, where it is ſaid, that *L. Veratius Feliciſſimus*, (*a*) patron of *Tolentium*, (or *Tollentium* a municipal town in *Italy*) gave to the inhabitants their annual ſacrifices, which were offered on the eleventh of May for a plentiful harveſt. That inſcription is cut on a braſs plate in the form of an oblong ſquare, with a female buſt in a pediment at the top, deſigned very probably to repreſent the deity, to whom they addreſſed. As the inſcription is peculiar in its kind, I ſhall here give the whole of it, as it ſtands in *Reineſius*.

TESSERAM.

(*a*) *Grut. Pag.* CXCIV. 2.

TESSERAM. PAGANICAM
L. VERATIVS. FELICISSI
MVS. PATRONVS. PAGANIS
PAGI ♡. TOLENTINES
HOSTIAS. LVSTR. ET. TESSER.
AER. EX. VOTO. L. DD
V. ID. MAIAS. FELICIT. (*a*).

This is called *teſſera paganica*, as I imagine, from its intitling the *pagani*, or inhabitants of that town, to the annual claim of the ſacrifices therein mentioned. And ſo far it agreed with the nature of a public *teſſera*, which being lodged in the hands of the proper officer, authorized him to collect the ſeveral contributions aſſigned for ſuch religious purpoſes. And of this latter ſort I take the plate to have been, which makes the ſubject of our preſent inquiry; both the form and ſize of it ſuiting very well with ſuch a deſign, as it was portable, and ready to be produced, if occaſion required. And agreably to this notion of the word *teſſera* the antient *Gloſſaries* interpret *teſſerarius* by γραμματεύς, a *ſcribe* or *clerk*.

As to the following word SEDIARVM, tho it no where elſe occurs, that I know of; yet this, I preſume, can be no juſt objection againſt its being taken here for the name of a town, called *Sediae*: when it is conſidered, how many inſtances of the like nature are to be found in the inſcriptions collected by *Gruter* and others, which give us the names

(*a*) Append. num. 8.

names of many antient places in the Roman provinces not mentioned by any other writers. And besides, the form of this word appears analogous to the names of several other Roman towns here in Britain; as, *Durobrovae* Rochester, *Ratae* Leicester, *Rutupiae* Richborough, *Spinae* Spene, and some others. It is not improbable, that this plate was found not far from the place, whose name it bears; and which might be situated among the *Cateuchlani*, as their territories are described by Camden (*a*). But as I have never before seen, nor heard of any thing similar to it, I would submit what is here offered to the judgement of the curious in these inquiries.

G. C. Feb. 25.
1747.

John Ward.

XIII. *An Account of a very learned* Divine, *who was born with* two Tongues; *communicated to the* Royal Society *by* Cromwell Mortimer *M. D.* & Secr. R. S.

Read March 10. 1747.

IN a MS. Account of the Life of the Rev. Mr. *Henry Wharton*, Chaplain to Archbishop *Sancroft*, written by himself, I have met with the following Passage:

" Mihi quidem ex utero materno exeunti duplex
" erat Lingua, utraque ejusdem figuræ ac magnitudinis;
" inferiorem exscindendam esse clamarunt mulieres
" obstetrices.

(*a*) *Britann. pag.* 275, *ed.* 1607.

"obstetrices. Verum id noluit mater puerpera.
"Pietati ejus obsecundavit fortuna. Lingua enim
"inferior paulatim emarcuit, et in exiguam pisoque
"haud majorem lingulam, quæ hodienum manet,
"contracta est. Lingua interim superior ad justam
"crevit magnitudinem, quamplurimis longis pro-
"fundisque sulcis distincta, an vulneribus laniata,
"dicam! quæ parallelo situ posita una cum lingua
"creverunt, neque unquam coitura esse videntur."
Nat. *Nov.* ix. 1664. Ob. 1694-5. *Mart.* 5. *Æt.* 31.

It appears by this Journal of himself that he was always infirm and sickly. *

XIV. *Upon the* Sounds *and* Hearing *of* Fishes, *by* Jac. Theod. Klein *R. P.* Gedan. *F.R.S.* or *Some Account of a Treatise, intitled*, "An "Inquiry into the Reasons why the Au- "thor of an Epistle concerning the Hear- "ing of Fishes endeavours to prove they "are all mute and deaf;" *by* Richard Brocklesby *M. D. F. R. S.*

Read March 10. 1747-8.

OUR Author in the first place classes them into two Orders, the first hath Lungs, the other is furnish'd with Organs analogous to Lungs, which we call Fish-Ears, or Gills: All the Whale-Kind, the Dolphin, Porpoise, and such-like, have Lungs. There are two Families of the second Class, to one of them belongs all that Tribe,

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* See the Account of *Margaret Cutting*, who speaks without a Tongue, in these *Transf.* No. 484, *p.* 621.

Tribe, which have one, two, five, or nine Air-Holes, at the Back, or Sides of the Head, or in their *Thorax*, in which concealed Gills are found: The other Family comprehends all Kinds of Fishes, whose Gills are usually placed on each Side the Back of the Head. Our Author's Antagonist alleges, that all Fishes of both Orders are equally deaf; but that all Naturalists except Mr. *Reaumur* are of a contrary Opinion, that Fishes hear distinctly.

Our Author begins with an Air of Ridicule, and shews how far the Letter-writer is ignorant of the various Opinions, modern as well as antient. Our learned Countryman Mr. *Ray* thinks to reconcile these, by allowing that some hear, while others are deaf; but the greatest Part allow that Fishes actually hear; and most, except *Scheuchzer*, seem agreed about the auditory Passages. But the Letter-writer denies they have any Organs of Voice, merely upon the proverbial Authority, *Mute as a Fish*; hence he concludes they are likewise deaf. But in Answer, 'tis replied, the spouting Whale hath all its internal Organs, precisely similar to the Organs of Voice in other Creatures, and therefore they may answer the same Purposes, nay actually serve this End: For when the Whales in the *Greenland* Fishery are struck, they roar frequently so loud, as to be heard at two *French* Miles Distance.

But some of the first Family of our second Class, as the Skate, Lamprey, Conger, and others, our Author hath heard utter some kind of Noise; and gives his Opinion, that most Sorts of cartilaginous Fishes can do the same. From Analogy he argues, that as no Beast, from the Lion to the meanest Ani-

mal, nor from the Eagle to the humming Bird, but can utter a Voice, ſo he thinks the ſame general Law is obſerv'd in the Oeconomy of Fiſhes: But at the ſame time our Author here ſeems to lay too much Weight upon what he ſuppoſes final Cauſes, and metaphyſical Arguments, which have in all Ages ruin'd Natural Philoſophy.

But the Letter-writer queries, whether Fiſhes may not be mute in our Air, and yet capable of ſome Voice in their own Element. Our Author takes the Noiſe which Carp and ſuch Fiſh make in hot Weather, on the Surface of the Water, to be a Voice: And this is moſt remarkable when the Male impregns the Row which the Female has before depoſited; yet this is often heard, when the Fiſh is 6 or 7 Inches under Water. Our Author further enumerates many foreign Fiſhes, and particularly our Smelt, which put alive into Vinegar hiſſes very audibly.

The Letter-writer had objected againſt Fiſhes, that they have no Occaſion for Hearing, becauſe they never copulate, as other Animals do: But our Author deſcribes the Manner of Whales, which is performed as that of other Animals; and obſerves, that they bring forth their Young alive: Theſe follow the Female, and ſuck Milk from the Teats, which are placed in them near the Organs of Generation; and in violent Storms the Dam takes her Off-ſpring into her Mouth, and protects them from Danger. This laſt is common to ſeveral of the Skate-kind.

The Letter-writer alleges, That Fiſh never ſleep; but our Author aſſures us, all ſuch as have Lungs do in the Night-time, thruſting up their Noſtrils into

the open Air. For others he cannot be positive, as their History is little known.

The Letter-writer premises two Questions; first, Whether Fishes have any Ears? or, If the Gills serve the same Purpose? and answers positively in the Negative to both: And therefore concludes they cannot hear. But our Author asserts, that Snakes, Frogs, Chameleons, and others of the Lizard-kind, actually hear, without any of the usual external *Apparatus* of Hearing. For though they want the Auricles and Ears, yet have they auditory Passages, by which Sound is convey'd, and even internal Organs, to which the *Meatus auditorius* reaches. But our Author farther asserts, that all the Whale-kind, and in general such Fishes as have Lungs, have likewise a *Meatus auditorius*, and the internal Organs of Hearing; and appeals to a public Dissection of a Porpoise, and another Fish of the Whale-kind, made by himself; in which the *Os petrosum*, with the other Parts of these Organs, had been separately shew'd; and calls in the concurrent Testimony of Dr. *Tyson*, in his Anatomy of a Porpoise.

Thus having satisfied us about such Fish as have Lungs, he goes on to consider the cartilaginous Species, such as the Skate, Ray, and kind of Lamprey, which have Organs of Generation, and copulate like Brutes; yet exclude the *Fœtus* while yet in the Egg-State: And this from Analogy, that these, and in general all other Fish, as they have Organs which serve them for Lungs, so they may have what answers in others to the *Apparatus* of Hearing.

In Proof of this he asserts, that all Kinds of Fish but these which have Lungs, are always found to

have

have Stones in their Heads naturally form'd, and invariably plac'd in the ſame Situation, being join'd to the contiguous Parts with Ligaments and Nerves, which take their Riſe from the Subſtance of the Brain; and having examin'd the Head of a Pike minutely with a Microſcope, he diſcover'd the auditory Pores in the Stones, and perſuades himſelf, that three Pair of Stones are to be referr'd to this Uſe; therefore concludes, as there is ſome Analogy in the Organs, that all Fiſhes in ſome meaſure hear.

The Letter-writer farther objects, that Water is not the Medium of Sounds; and though Air is actually contain'd in all Water, yet it cannot be put into Undulations, any more than the circumambient Water; but that would require a much greater Vibration than the external Air can give. Thus, ſays he, if a Perſon immerge his Head a Foot under Water, he will hear nothing but a boiling Din; and however great a Noiſe is made in the open Air, the Event will be ſtill the ſame; and if the Water itſelf be put into the moſt violent Agitation, the Perſon will diſcover no Odds in that Senſation of his Ears from what he perceived in the ſtilleſt Water. Hence he concludes Water incapable of tranſmitting Sounds. Our Author replies, That as Fiſhes are unanimouſly agreed to be capable of ſmelling, ſo, by Analogy, it is probable they have Hearing; for Odours are convey'd by the Air, as well as Sound. But he thinks the unnatural Poſition of a Man's Head immerg'd a Foot under Water may be ſome Cauſe for that confuſed Noiſe, and oppoſes the experimental Teſtimony of Abbè *Nollet* himſelf, who went different Depths under Water, to ſatisfy

himself how far Sounds could be convey'd in that Medium.

At four Inches under Water he heard the Sound of a Gun discharged, of a Clock striking, and of a Hunter's Horn: These, repeated at different Depths, were heard first at 4, then at 8, afterwards at 18 Inches, and lastly at two Foot. A Man's Voice was also heard in the same Manner.

At different Altitudes of Water, none of them exceeding two Feet, he could perfectly distinguish mixt Sounds, when two Bells were struck, or two Pipes sounded together.

He could distinguish under Water, very distinctly, Words utter'd aloud: And prov'd this Assertion, by declaring, when he came above Water, what was said while he was under it.

All Sounds were heard more faintly, and attenuated; yet the Difference of the Sound, at 4 and 18 Inches Depth, was not answerable to the Difference of the Altitude of Water.

He observ'd at first, that momentary Sounds were not so well convey'd as continu'd; yet he afterwards determin'd, at the same Depth, one Tap of a Drum-head, as plainly as a continued Round. This he thinks was the same in a Man's Voice, and the Sound of a Pipe; but ingenuously owns, he was not fully satisfied in this Experiment; and therefore does not lay as great Stress on its Certainty as on the former.

Lastly, he held his Head under the Surface of the Water, so as barely to cover him; but could not hear the Clock strike, which was audible in the open Air at 45 Feet Distance, especially on a Plain.

The

The Abbè therefore concludes, if Fishes do not actually hear, 'tis for want of proper Organs, and not because the Medium cannot convey Sounds.

Our Author mentions the common Notion of Carp, and other Fish, coming out of their Holes at the Sound of a Bell to be fed; and adds a Story, which Mr. *Boyle* somewhere relates, that near *Geneva* a Man had a Fish-pond, whose Banks were so high from the Plain on which it was, that one could not look over them into the Pond; and therefore it was impossible the Fish could see the Person; yet they were at any time conven'd at certain Sounds by the Gardener, in order to be fed, as a creditable Person asserts.

The Letter-writer, having made a high Partition in a Pond, watch'd while an Accomplice behind it made a very great Noise, and discharg'd a Gun, in order to frighten the Fish (if possible) that were playing on the Surface of the Water; but they did not give any Attention; yet as soon as ever they came in Sight, the Fish immediately made off.

Our Author thinks this Objection of little Weight, because the Question is not, whether Fishes, when they see nothing, can be frightened by Sounds only.

Upon the Whole, our Author shews himself an experienc'd and diligent Naturalist, and will (if I mistake not) be allow'd to have fully prov'd the Falsity of any Assertion, that all Fish are intirely mute and deaf.

XV. *An Account of the* poisonous Root *lately found mixed among the* Gentian; *by the same.*

Read March 17. 1747.

THE following Account is the best I have receiv'd of the poisonous Effects of a noxious Root, lately found in a Parcel of Gentian, and exhibited for Use to several Persons instead of it. And as it is attended with such dangerous Consequences, I thought even an imperfect Relation of Facts had better be given immediately, than to expect more Circumstances, and wait so long for them, till greater Mischiefs might happen, by the Inattention of such as are constantly administring Medicines. The following Account was sent by a Gentleman of *Hambleden* Parish, *Buckinghamshire*; and is found to agree in general with some other fatal Instances that have happen'd since in *London*.

Mary Burgess, aged 60 Years, about 5 o'Clock in the Morning, drank of an Infusion of only one Penyworth (without other Ingredients) of supposed Gentian-Root, in half a Pint of white Wine: It is uncertain what precise Quantity she took; but in two Hours afterwards she falter'd in her Speech, had Twitchings and Convulsions of her Hands so far, that the ignorant By-standers alleg'd the poor Woman was drunk; and so left her a bed till 12 o' Clock, to sleep it out. On their Return however she appear'd much worse, was speechless, and remain'd so 3 whole Days, and did not know any body all that time. In her Illness a Purging came on, and at last carried her off.

Katharine

Katharine Woodward, aged 44 Years, took about a Tea-ſpoonful of the ſame Wine, and ſoon after fell down ſpeechleſs, and her Limbs were paralytic near 36 Hours: After that ſhe recover'd her Speech, but continued ill above a Fortnight, and Part of that Time her under Jaw was convulſed, and ſhe bled both at Mouth and Noſe, in the Beginning.

Mary Diggins, aged 33 Years, taſted a much leſs Quantity of the ſame Wine than the former had done; and though terrify'd at her Neighbour's bad Symptoms, ſhe drank warm Water with Oil, in order to vomit; yet ſhe ſoon ſtagger'd, and grew delirious, could not ſwallow any Solids, and loſt the perfect Uſe of her Eye-ſight a Fortnight.

The vague Reports of theſe, and Mr. *Pots*'s Caſes induc'd me to obtain the Favour of two or 3 Druggiſts to look over ſome Gentian-Root, one Parcel of which had no leſs than a 20th Part of a Root, which at firſt Sight was diſcover'd to be no Gentian.

This Root, for which we have yet no Name, is of a greyiſh brown Colour externally, but it is browner, and more reſinous internally: Moſt of that which I have ſeen, is about the Thickneſs of a Finger; tho' ſome is much larger and whiter; which is a Reaſon with ſeveral for thinking there are two Sorts of it; and indeed ſome Pieces emit a ſtronger and more nauſeous Smell: But this I apprehend may be occaſion'd only by a larger Quantity of Reſin in them. All of them are of an acrid pungent Taſte, and leave a Dryneſs on the Tongue.

I judg'd

I judg'd it therefore neceſſary to try what Effects this Root might have on Dogs, that I might thereby the better conjecture concerning them on the human Species; and though no Man has any Right wantonly to torture or deſtroy in a cruel manner the leaſt Animal; yet when good Purpoſes are anſwer'd in the Whole of Things by inferior Natures yielding to ſuperior ones, a Man may, without juſt Imputation to his moral Character, ſacrifice the Intereſt of a baſer Order to the Happineſs of one ſuperior.

With this Intention I decocted half an Ounce of this unknown Root, powder'd groſſly in ten Ounces of fair Water, till two were evaporated; then let the Decoction ſtand 6 Hours. After this I gave half of it, ſtirring up the Powder, to a young Dog. This made him inſtantly foam at the Mouth; he grew ſick, and vomited Part of the Doſe; yet in leſs than half an Hour reel'd like one drunk, had Twitchings of his Limbs, and after ſome time the Motion of his Heart was irregular, and intermittent, though ſtrong: He was ſleepy about an Hour, but came gradually to himſelf in half an Hour more, and eat Victuals, which before he refus'd.

Two Days after, the ſame Dog took 4 Ounces of Decoction of Gentian made as ſtrong as the former; but I diſcover'd not any bad Symptom from it. I uſed this Quantity, as Gentian-Root is ſometimes given to that Quantity in the Practice of Phyſic. It is above ten Days ſince he took the firſt Decoction; and hitherto continues well.

Another Dog took above a Dram Weight of the unknown Root, finely powder'd, and mix'd with Butter:

Butter. It inſtantly made him foam from the Mouth, and caus'd ſudden Vomiting, and, in half an Hour, Weakneſs of his Limbs, and Staggering, which laſted half an Hour, and then he recover'd.

I tried to give a larger Quantity to another Dog; but it being too much like other irritating Medicines, cauſed ſo great a Vomiting, as deſtroy'd the Effects which a ſmaller Quantity had before produc'd.

One of the Dogs had ſome looſe Stools after taking it; another urin'd plentifully.

Like Experiments have been made by Mr. *Pearce* at St. *Thomas*'s Hoſpital, which had nearly the ſame Event.

Though none of the Dogs were killed by this Drug, but remain to Appearance well, yet all Apothecaries have ſufficient Reaſon to examine very ſtrictly their Gentian, and to reject what they find not genuine, ſince one of the Women before-mention'd, and a Man that I have heard, of are both dead; and ſince Gentian is of general Uſe in medical Compoſitions, as well as the primary Ingredient in the cordial Bitters Ladies make for their own Uſe.

XVI. *An Account of large* ſubterraneous Caverns *in the Chalk Hills near* Norwich; *by Mr.* Wm. Arderon, *F. R. S. compriſed in a Letter from Mr.* Henry Baker *F. R. S. to the* Preſident.

Read March 24. 1747-8.

AS the Inſpection and Study of Nature is the particular Province of the *Royal Society*, and every Attempt to improve our Knowledge is certain of your Favour, I take the Liberty to lay before you the Subſtance of a Letter from my induſtrious Correſpondent and Friend Mr. *William Arderon*, F. R. S. containing the Deſcription of a large Vault or Cavern, extended under ſeveral Hills near the City of *Norwich*, with ſome Obſervations and Experiments made by him there.

About a Quarter of a Mile from the City of *Norwich*, on the Eaſt Side thereof, and near the Entrance of *Mouſhold-Heath*, is a large ſubterraneous Cavern, which has been formed in a long Series of Time, by the digging out of Chalk for the making of Lime. There's but one Entrance into it, whoſe Breadth is about two Yards, and its Height nearly the ſame; however the Height gradually riſes, till at laſt it meaſures in ſome Places from twelve to fourteen Yards. But notwithſtanding the Entrance is ſo ſmall, the whole Area within is of ſuch a large Extent, that twenty thouſand Men might with great Eaſe be plac'd therein, as I believe will ſcarcely be doubted, when I aſſure you, that, from the Entrance to the furtheſt Part of theſe darkſome Cells, meaſures full four hundred Yards; and that theſe Paſſages are frequently ten or twelve Yards wide, with Branchings out

out on the Sides, into various Lanes and Labyrinth-kind of Windings, that every now and then open into one another; which renders it no easy Task to find the Way out, when a Person has been a little bewilder'd in these subterraneous Mazes.

Most of these Vaults are arched at Top, whereby the immense Weight, which every Moment presses on them, is well supported; a Weight no less than that of Hills, whose perpendicular Altitude above the Tops of these Arches is twenty or thirty Yards, if not much more. I have frequently, says my Correspondent, gone into these Caverns out of Curiosity; but could never perceive the least Appearance of those Damps* which are so common in Mines, and other subterraneous Places, where the Air is stagnant for want of a due Current; which should seem to be the very Case here, as there is but one Entrance into it. The Passage indeed is horizontal, and open to the West Wind; but the included Air's being free from Putrefaction, may possibly be owing to the large Quantity of Salt which the Chalk contains.

How deep or thick these Rocks of Chalk are, no one, so far as I can find, can tell; for, in sinking the lowest Wells, they have never, that I know of, been dug thro'; and consequently must be exceeding deep. The Chalk at the further End of this Cavern is so very soft, that it may be moulded with the Hand like Paste; which I take to be its original Consistence, and what it always retains, till it becomes expos'd to the Air. In the very lowest Parts of

* Those are commonly caused by sulphureous Vapours, which never appear in Chalk. *C. M.*

of theſe Vaults I have pick'd up ſeveral Kinds of Foſſils, figur'd by marine Bodies; ſuch as *Echini*, *Pectunculi*, common or fluted Cockle, *Belemnitæ*, *&c.* and, by diligent Search, other Sorts might perhaps be found. Sounds made beneath theſe arched Roofs are ſtrongly reflected from Side to Side; ſo that the leaſt Whiſper may be heard at a conſiderable Diſtance. The Beat of a Pocket-Watch was heard diſtinctly full twenty Yards from where it was plac'd.

I viſited this Place on the 1ſt Day of *November* laſt, in order to try the Temperature therein, as to Heat and Cold; and carried with me a Thermometer regulated by one of Mr. *Hauksbee*'s, which I ſet down at the further End of theſe Caverns; and letting it remain there for ſome time, I found the Mercury reſted at 52°. which comparing with the Regiſter I had kept, was, I found, within half a Degree of a Medium betwixt the greateſt Heat and the ſharpeſt Cold we have known in this City for ten Years paſt; and it is very probable, if the two Extremes had been taken more exactly, the Temperature in theſe Caverns would be found to come yet nearer to the Medium of Heat and Cold in this Climate.

	Hauksbee's *Therm.*
The greateſt Degree of Heat was *July* 18. 1746.	15
The greateſt Degree of Cold was *Jan.* 9. 1740.	88
Which added together make ——— ———	103
The Medium of which is ——— ———	51½

I find,

I find, by inſpecting Mr. *George Martin's* Collection, and Compariſon of the Scales and Degrees of Heat with various Thermometers, that the Temperature of Heat in theſe Caverns coincides with that in the Cave at the Obſervatory at *Paris*, within one Degree; which I think comes very near, conſidering the Obſervations were made with different Inſtruments, and formed upon different Principles.

At the Foot of a high Hill, adjacent to theſe Vaults, iſſues out a curious Spring, whoſe Water I found exactly of the ſame Temperature with that underground; though, when the Thermometer was expoſed to the open Air, it ſtood at 57°.

Permit me, Sir, to ſubſcribe myſelf, with the utmoſt Truth and Reſpect,

London, March 15. 1747-8.

Your moſt obedient

humble Servant,

Henry Baker.

A terrible Thunder-Storm, *June* 12, 1748, ſhook the Earth to ſuch a Degree as to throw down thoſe Chalk-Vaults.

XVII.

XVII. *An Account of ſome* Obſervations *and* Experiments *made in* Sibiria, *extracted from the Preface to the* Flora Sibirica, *ſive* Hiſtoria Plantarum Sibiriæ cum tabulis æri inciſis. *Auct. D.* Gmelin. *Chem & Hiſt. Nat. Prof.* Petropoli 1747. 4*to. Vol.* I. *by* John Fothergill, *M. D. Lic. Colleg. Med. London.*

Read Feb. 11. 1747-8.

BY Direction of the late Empreſs of *Ruſſia*, ſeveral Members of the *Royal Academy of Sciences* at *Petersburg* undertook a Journey into *Sibiria*, in order to inquire into the Natural Hiſtory of that Country, and to make ſuch Experiments and Obſervations, as might tend to give a juſt Idea of that almoſt unknown Region, and to the Improvement of Phyſics in general.

Dr. *John George Gmelin*, Profeſſor of Chemy and Natural Hiſtory at *Petersburg*, was ſent at the Head of this Deputation, who, beſides ſeveral of his Collegues, and ſome Students, had a Painter or two, a Miner, Huntſman, and proper Attendants in his Retinue.

He ſet out upon this Expedition in *Auguſt* 1733. and returned to *Petersburg* in *Feb.* 1742. after having ſpent nine whole Years in viſiting almoſt every Part of *Sibiria*.

The Fruits of this Undertaking are deſigned to be communicated to the Pubilc; and one Volume

 of

of the Hiſtory of Plants has already appeared, under the Title of *Flora Sibirica, ſive Hiſtoria Plantarum Sibiriæ, Tom.* I. *continens Tabulas Æri inciſas L. Auctore D.* Joh. Geo. Gmelin. *Chem. et Hiſt. Natur. Prof.* Petropoli *Typis Academiæ Regiæ Scientiarum* 1747. This is intended to be followed by ſeveral others, containing a not only a Deſcription of the Plants, their *Locus natalis*, &c. but their Uſes amongſt the Inhabitants, ſo far as the Profeſſor could get Information concerning them.

In a large Preface to this firſt Volume, the ingenious and indefatigable Author has given us a conciſe Account of *Sibiria* in general, its Rivers, Lakes, Mountains, Mines, the Nature of the Soil, Fertility, *&c.* with ſeveral judicious Experiments and Remarks on the Altitude of the Earth above the Level of the Sea; but eſpecially on the Qualities of the Air in that Climate; an Abſtract whereof, at firſt drawn up for private Entertainment, was thought not unworthy of more public Notice, and is therefore addreſſed to the *Royal Society*.

The Country, whoſe Natural Hiſtory D. *Gmelin* has collected, is of vaſt Extent: It is bounded by a Chain of Mountains called the *Werchoturian* and *Vralian* on the Weſt; by the Sea of *Kamtſchatka* on the Eaſt; and comprehends all thoſe Countries that lie betwixt the *Mare glaciale*, and the Borders of the *Kalmucks* and *Mongales*, to the very Confines of *China*.

The Rivers which water this Tract are numerous; ſome of them large, and even receiving Streams in their Courſe, which in other Countries would be looked

looked upon as Capitals themſelves. The Space they meaſure is no leſs conſiderable. The *Jaik* is the firſt River of Note on the Weſtern Side. It riſes under the Latitude of 54, of Longitude 78, and runs into the *Caſpian* in 47 of Latitude, and 74 of Longitude. The *Irtiſch* riſes in the Country of the *Kalmucks*, Lat. 46½, Long. 103; and empties itſelf into the *Oby*, Lat. 61, Long. 86. The *Oby* riſes under 52 Lat. 103½ Long.; and loſes itſelf in the *Mare glaciale*, Lat. 67, Long. 86. after running a Courſe of near 800 Leagues, and receiving a great Number of Rivers of conſiderable Note. The *Jeniſea* is not much leſs than the *Oby*. The *Selenga* takes its Riſe under Lat. 48, Long. 114; runs into the Lake *Baical*, in 51° 20″ Latitude, with many others equally conſiderable, which it would be tedious to mention.

The Water of theſe Rivers is for the moſt part freſh, clear, and ſalubrious: In ſome it is a little brackiſh, by the Mixture of Currents from ſalt Lakes and Springs, which abound in many Places: They contain Fiſh of various Kinds in great Plenty, and moſtly of an excellent Flavour.

The Lake *Baical* may deſerve ſome Mention to be made of it, being one of the greateſt freſh water Lakes yet diſcover'd: It extends, according to our Author, from the one hundred and firſt Degree of Longitude, to the one hundred and twenty-ſeventh, being upwards of 500 Leagues in Length, and is from twenty-five to eighty Leagues in Breadth. It is every-where deep and navigable; the Water is extremely clear; it abounds with great Plenty of

fine

fine Fiſh: It receives a great Number of Rivers, but the *Angara* alone runs out of it; which joining the *Tunguſca*, loſes its Name; as this likewiſe does, when it runs into the *Jeniſea*.

Salt Lakes are common in many Parts of *Sibiria*; ſome contain a pure white Salt, well-taſted, and fit for Uſe; which, in Summer, is chryſtalliſed by the Heat of the Sun alone, and forms a Cruſt on the Top of the Lake. In ſome, this grows ſo heavy as to break, and fall to the Bottom. Beſides this kind of pure common Salt, which is fit for Uſe, there is another Sort of a bitter Taſte, much reſembling the *Sal mirabile*, found in ſeveral Lakes in this Country. Springs of ſalt Water are ſometimes obſerved to riſe in the midſt of freſh Water: Our Author aſſures us, that he has ſeen ſeveral ſuch; one eſpecially he obſerved riſing thro' a Stone, in the Bed of the River *Angara*.

Before we diſmiſs the ſalt Lakes, we may juſt mention, that on the Banks of the River *Kaptendei*, where it runs into the *Wilvius*, are a great Number of ſalt Springs, which afford excellent Salt; and that, about 30 Leagues above this Place, along the ſame *Kaptendei*, on the right Hand, is a Hill about 30 Fathom high, and 210 long, conſiſting intirely of *Sal Gem*.

There are ſome Lakes, which, our Author informs us, in the Memory of Man, contained only freſh Water, but are now very ſalt. One of this kind, about 40 Years ago, abounded with freſh Water Fiſh, but is now become ſalt, ſmelling ſtrong of Sulphur, with a bitter Taſte, and all the Fiſh are killed.

The Inhabitants aſſured our Author, that ſome freſh-water Lakes have been by degrees dry'd up, and that others have appeared, where formerly it was dry Ground; and that even ſome of theſe new-formed Lakes, which at firſt had no Fiſh in them, are now very plentifully ſtock'd. They have not recourſe to ſubterranean Caverns or Paſſages, for a Solution of this *Phænomenon*; but aſſert, that Ducks, Sea-Mews, *&c.* that live upon Fiſh, carry the Eggs from one Lake to another.

In the Deſcription which our Author gives us of the Courſe of Rivers, Situation of Lakes, *&c.* he takes notice of the Soil, its Barrenneſs, Fertility, *&c.* Theſe are different, as it may be ſuppoſed, in the different Parts of ſuch an extenſive Climate under ſuch Latitudes. About the Lake *Baical* is the moſt fruitful Tract, and thence is called the Granary of that Part of *Sibiria.* They grow ſome little Corn about the Latitude of 61. They have made of late Trials ſtill further; but the Succeſs was not known.

In his Paſſage thro' *Sibiria,* he tells us, that he could ſcarce think himſelf in *Aſia,* till he got over the River *Jeniſea:* Till then, he ſaw no Animals, but ſuch as are common in *Europe,* at leaſt may be ſeen in the Plains waſhed by the lower Part of the *Volga:* The Plants and Stones were of the ſame kind, and the Face of the Country in general, like other Parts of Northern *Europe.* But from the *Jeniſea,* both to the Eaſt, North, and Weſt, the Climate ſeemed to be wholly different, and as if it were enlivened with new Vigour. It is mountainous; but theſe Mountains are intermixed with rich

delightful

delightful Valleys, and fruitful Plains. The Animal that affords the Musk, and the *Musimon* of the Ancients, were now to be met with. Many of the most common *European* Plants by degrees disappeared, and others became frequent, which are Strangers in *Europe*. The Purity, Clearness, and Salubrity of the Waters, the exquisite Taste of the Fish and Fowl, but more especially the different Genius and Way of Life of the Inhabitants, plainly proved they were got into another Climate. This Remark our Author submits to the Consideration of Geographers.

Amongst the Curiosities of *Sibiria* the Professor mentions a Place remarkable for its excessive Coldness in the midst of Summer. It is in the Province of *Jacutski*, about the middle Way to *Ochotz* along the River *Junacan*; it is called by the *Russians* *springing Ice*, by the Natives the *icy Lake*. Three other such Places occur within the Circuit of eighty Leagues.

The Provinces beyond the Lake *Baical* are mountainous, with high and wide-extended Plains lying betwixt them, which in many Places are only cover'd with barren Sand; so that in some Places one may travel thro' such Deserts one, two, or three Days together, without finding Wood enough to make a Fire, or any other Water than that of salt Springs, which are very frequent; and being dried up by the Summer-Heats, leave a saline Crust, very much resembling *Natron*, being of an alcaline Nature, with a sulphureous Smell.

The Country that borders on the Rivers *Uruncan* and *Gasimur* is extremely rich and fruitful. The

Face of the Country is delightful, and its Produce to the Husbandman almost exceeding his Hopes: But what renders it still more surprising, is, that a Country, whose Soil yields to few in Fertility, and the Beauty of its Bloom, should yet cover immense Riches in its Bosom. Here are Mines of Gold and Silver, which have long been worked to Advantage: The Veins are rich, and lie shallow; yet communicate no poisonous *Effluvia* to the Vegetables that cover them: Nor do those distinguishing Marks of Sterility appear here, which in most other mining Countries are so observable.

The highest Part of *Sibiria* is towards the Springs of the Rivers *Argun*, *Schilca*, &c. about the 49th Deg. of Lat. 130th Longit. This Part is destitute of Marble and Lime-Stone, which are almost every-where to be met with in the lower Tracts both of *Sibiria* and *Russia*: No Petrifications are to be found here, either of the testaceous or crustaceous Animals: And the Veins of Ore are always found near the Surface, never entering deep into the Earth. Besides the Mines of Gold and Silver above-mention'd, Copper and Iron are found in several Places; likewise the *Glacies Mariæ* or *Muscovy Glass* is dug near the River *Mama*. Loadstones are also got in *Sibiria*; and in several of the Rivers beautiful transparent Pebbles and Chrystals occur.

I shall only add, that there are some natural warm Baths in several Parts of *Sibiria*, and some of them of a most agreeable Temperature; and proceed to the Account of our Author's Observations and Experiments on the Height of the Earth, *&c.*"

Pauda

Pauda is allowed to be the highest of all that Ridge of Mountains called *Werkoturian*. Our Author endeavoured to take the Height of it by means of the Barometer.

On the 11th of *December* 1742, at our Author's Lodgings at the Foot of *Pauda*, the Mercury in the Barometer, in a cold Place, but within-doors, stood at $26\frac{83}{100}$ *Paris* Measure. He then carried it up the Mountain as high as he could go, which was about one Third of the whole Height, where he hung up the Barometer on a Tree, from 9 to 11 in the Forenoon, making a good Fire pretty near it, lest the intense Cold, which sunk the Quicksilver in *De Lisle*'s Thermometer to 201, should affect the Barometer, and lead him to ascribe that to Gravity, which was only owing to the Contraction of Cold.

Under these Circumstances the Quicksilver sunk to $25\frac{32}{100}$.

Hence, according to M. *Cassini*'s Calculation, our Author' first Station will be 941 Feet higher than the Level of the Sea: The second on *Pauda* 1505 f. and the whole Height of this Mountain 4515, or 752 *Paris* Toises; which, added to 941 Feet, the Height of his Lodgings at the Foot of *Pauda*, makes 5456 Feet, or 909 Toises, the Height of *Pauda*'s Top above the Sea; supposing the Level of the Sea to be 28 Inches, as the *Paris* Academicians have fixed it: Tho' this differs from Observations made on the Barometer at the Sea-coast of *Kamschatka* at *Bolcheretz*; where, from Experiments made for above two Years, the mean Height of the Mercury was 27 Inches, $6\frac{1}{2}$ Lines. And at *Ochotz*, during a Year's Observations, the mean

mean Height was found to be 27 Inches and about $8\frac{1}{2}$ Lines.

Hence it would appear, that the Sea of *Kamtſchatka* is higher, with reſpect to the Earth's Centre, than the Ocean and Mediterranean; and at *Bolcheretz* higher than at *Ochotski.*

The following Liſt of barometrical Obſervations, made in various Parts of *Sibiria*, will ſhew the different Heights of the different Tracts in it.

	Feet	Toiſes	Inches
The mean Height of the Barometer, from a Year and 10 Months Obſervations at *Ircuts*, was —— ——		——	$26\frac{38}{100}$
Its Height above the Sea will then be —— ——	1355	or 226	——
At *Selengia*, 1 Month's Obſervations, —— ——		——	$25\frac{95}{100}$
Its Height above the Sea	1779	or 296	——
At *Kiachta*, a Town on the Confines of *China* 12 Days Obſervations in *April* and *May*, mean Height ——		——	$25\frac{35}{100}$
Its Height	2400	or 400*	

At

* In the Copy before me appears to be a great Miſtake, either of the Printer, or in the Manuſcript; it being put down in Words at Length, *bis mille quadringentarum Orgyarum cum dimidia*; which is impoſſible; and the Number of Feet is not exact, according to other Calculations.

	Feet	Toises	Inches
At *Nertschia*, from 20 Days Observations in *June*,	—	—	$25\frac{99}{100}$
The Height above the Sea	1738 or	298	—
At the Silver-Mines at *Argun* 9 Days in *July*, —	—	—	$25\frac{62}{100}$
The Height above the Sea	2121 or	$353\frac{1}{2}$	—

Our Author adds several judicious Reflections upon the Time and Manner of making these Observations, in order to determine any thing with Certainty; which he has endeavour'd to keep strictly to in these Experiments; and concludes, that the Plains in some Parts beyond the Lake *Baical*, are almost as high as the Tops of high Mountains in some other Countries; Mount *Massane*, according to the *French* Geometricians, being but about 408 Toises high; which differs but little from the plain Country at *Kiachta*; which yet has considerable Mountains rising in its Neighbourhood.

From whence our Author concludes, that the Elevation of the Earth, in this Tract, above the Level of the Sea, is very great, compared with the West Part of *Sibiria* and *Europe*. *

The

* M. *De la Condamine*, in his Voyage thro' the inland Part of *South America*, makes *Quito* to be between 14 and 1500 Toises above the Level of the Sea. Suppose ——— 1450
He tells us, that *Pichincha* is 750 higher — — 750
This makes in the Whole — - - - — 2200 Toif. above the Level of the Sea.

P. Martel, Engineer, in his Account of the *Glacieres* in *Savoy*, printed at *London* 1742, tells us, that the Barometer at *Geneva*, by the Side of the *Rhone*, stood at $27\frac{2}{12}$ I. which is 656 Feet above the Level of the Sea according to *Scheuzer*; and that the highest Point of

The Air of *Sibiria*, with respect to its Gravity, is, as in other Countries, the nearer the Sea the heavier; and the more remote, the lighter: So that at *Kiachta* scarce one Person in our Author's Retinue escaped without some Indisposition: They were seized after their Arrival, some with acute Fevers, others complain'd of extreme Lassitude and Dejection. It was in the Spring-Season, the Weather moderate, their Manner of living regular, nor had they been much fatigu'd with their Journey; in short, they could attribute it to no other Cause than the Lightness of the Air.

In these Provinces, *viz.* beyond the Lake *Baical*, our Author tells us, that Intermittents are seldom heard of, and Ophthalmies are endemic: But that, in the fenny Tracts which lie near the *Oby* and *Jenisea*, intermitting Fevers are very frequent.

The Coldness of the Air of *Sibiria* is of all others the most remarkable Quality. In some Places it snows frequently in *September*, and not seldom in *May*: In *Jacutsk*, if the Corn is not ready to cut in *August*, which often is the Case, the Snow sometimes prevents it, and buries the Harvest all together. At *Jacutsk* the Professor order'd a Hole to be dug in the Earth, in a high open Place, on the 18th of *June*; the Mold was 11 Inches deep; below that was Sand about 2½ Feet; it then began to feel hard, and in half a Foot more it was froze as hard as possible.

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of *Mont Blanc*, measured partly by the Barometer, and where inaccessible from the Snow that covers it, by trigonometrical Operations, is 12459 Feet, or somewhat more than 2076 Toises above the Level of the *Rhone*; which, added to the Height of this above the Sea, makes 13115 *French* Feet, or about two *English* Miles and two Thirds.

poſſible. In a lower Place, at no great Diſtance from this, he order'd another Hole to be dug: The Soil was 10 Inches; ſoft Sand 2 Feet 4 Inches; below this, all was congealed; ſo that the Earth is ſcarcely thaw'd even in Summer above four Feet deep.

Our Author inclines to the received Opinion, that the Eaſtern Climates under the ſame Latitude are colder than the Weſtern; and thinks this is confirm'd by Experiments made in different Parts of *Sibiria*.

The Mercury in *De Liſle*'s Thermometer often ſunk in Winter in very Southern Parts of this Country, as near *Selinga*, to near 226, which is equal to $55\frac{1}{2}$ below 0 in *Fahrenheit*'s Thermometer. But the Cold is often much more intenſe than this, as appears by the following Experiments, made at *Kirenginski*.

Feb. 10. 1738. at 8 in the Morning the Mercury ſtood at 240 Degrees in *De Liſle*; which is 72 below 0. in *Fahrenheit*'s. On the 20th it ſunk one Degree.

At the ſame Place in 1736.

Decemb. 11. at 3 in the Afternoon 254 in *Deliſle*. Almoſt 90 below 0. in *Fahrenheit*.

Decemb. 20. 4 o' Clock *p. m.* 263 in *Deliſle*. $99\frac{44}{100}$ below 0. in *Fahrenheit*.

		D.	F.
Novemb. 27.	12 at Noon	270 =	$107\frac{73}{100}$ below 0.
Jan. 9.		275 =	$113\frac{65}{100}$
1735 *Jan.* 5.	5 in the Morn.	260	
	6 ——	280 =	120
	8 ——	250 and roſe by degrees	

till 11 at Night, when it ſtood at 252.

Such

Such an Excefs of Cold could fcarcely have been fuppofed to exift, had not Experiments, made with the greateft Exactnefs, demonftrated the Reality of it.

During this extreme Froft at *Jenifea*, the Magpies and Sparrows dropp'd down as they flew, and to all Appearance dead; tho' they moft recover'd when brought into a warm Room. This was quite new to the Inhabitants of that Country; tho' it frequently happens in *Germany* in much lefs intenfe Cold, when the Weather fets in at once very fevere.

The Air, fays our Author, was at that time extremely unpleafant; it feemed as if itfelf was froze, being dark and hazy; and it was fcarce poffible even to bear the Cold in the Door-Way for three or four Minutes.

Thefe Experiments, our Author affures us, were made with all poffible Exactnefs, and agree with many others, made in different Parts of *Sibiria* by his Direction; and from thefe we may conclude that the Cold in *Sibiria* is more intenfe than it has yet been found to be in any other Part of the World.

It was not apprehended that a greater Degree of Cold exifted any-where, than that artificial one produced by *Boerhaave*, by means of concentrated Spirit of Nitre, which funk the Mercury 40 Degrees below o. in *Fahrenheit*'s; which was fuppofed to be the Point beyond which no Animal could bear it.

But the utmoft Limits of Cold are yet unknown; or to what Degree an Animal can fubfift in it, when inured to it by little and little. The Hiftory of Heat is alike imperfect. The celebrated Profeffor above-

above-mention'd was induced to think, that a Man could not bear, without the utmoſt Danger, a greater Heat than that which would raiſe the Mercury to 90 in *Fahrenheit's*; but an ingenious and accurate Correſpondent of our Author's at *Aſtrachan* informs him, that it not only riſes there to this Degree frequently, but even to 100, and he has ſeen it $103\frac{1}{2}$. Even in the Bagnio's in *Ruſſia*, the Heat is often equal to 100: It ſometimes makes the Quickſilver aſcend to 108, 10, and to 116, as may be tried every Day; and yet People not only bear them with Impunity a few Minutes, but often ſtay half an Hour or an Hour.

One neceſſary Obſervation our Author makes, which is, that the Ball or Tube containing the Mercury ought to be as dry as poſſible on the Outſide, during theſe or any other Trials with the Thermometer: For the adhering Moiſture, by forming a cooler Atmoſphere around it, has ſometimes occaſion'd a Difference of 10 Degrees.

Theſe are ſome principal Facts given us by our Author in his Preface, relative to the Natural Hiſtory of *Sibiria* in general: What follows chiefly regards the Work it is prefixed to.

As a juſt Idea of this Part cannot be exhibited in a narrow Compaſs, the Curious in this Branch of Science muſt be referr'd to the Book itſelf.

I have only to acknowledge with Gratitude the Inſtruction and Entertainment I have received from this elaborate Work: It is a Tribute juſtly due to the learned and ingenious Author, in Return for the Pains he has taken, and the Fatigue he has endured in this inhoſpitable Region; and to intreat

your Indulgence, if I have flatter'd myſelf too much, in apprehending this Excerpt might afford you ſome Amuſement.

XVIII. Novum reique medicæ utile *Electricitatis* inventum exponit *Joannes Henricus Winkler*, Profeſſor *Lipſienſis*, et *Societatis Regalis Londinenſis Sodalis*.

Lipſiæ, die Martii 12, 1748.

Read March 31. 1748.

SUbtiliter dividendi vim habet Electricitas. Quas vero ſolvit materias, earum partes ſecum abripit, et in loca transfert, in quibus ſcintillæ electricæ exiſtunt. Res odoras in vitreis vaſis bene naviterque concluſas et munitas ita diſcerpit, ut oriundæ exhalationes æque facile, ac vis magnetica, vitrum penetrent, et per atmoſphæram cylindrorum et catenarum, quibuſcum electricitas communicatur, inſtar fluminis dimanent. Quæ ex altera cylindri extremitate egreditur, materia electrica accedentem manum odore aromatico inficit. Non autem perſtat odor communicatus in hac corporis parte, quam electricum flumen afflavit: ſed, continuata adſpiratione, odorifera materia univerſum corpus humanum pervadit. Non modo cutis et veſtimenta fragrant, ſed aer, quem pulmones reddunt, et ſaliva, et ſudor hominis imbuti redolent aromata, quæ in vaſe obturato electricitate agitata ſunt.

Inopinatæ huic virtuti fidem faciunt obſervationes et experimenta, quæ ſenſu animoque attento capta

ſunt.

ſunt. Anno 1747, lagenam vitream aqua implevi, in eaque nitrum ſolvi. Immota ſtetit hæc lagena per aliquot hebdomadas. Limpida igitur facta erat aqua, poſtquam nitri partes graviores fundum petierant. Sub finem anni in hanc limpidam aquam immiſi filum metallicum, idque cum aliquo tubo metallico ex filis ſericis ſuſpenſo conjunxi. Sub iſto tubo diverſis temporibus jam metalla, jam vaſa metallica aquis repleta, in quibus ſphæræ vitreæ minutiis metallicis impletæ locum habebant, collocavi. His adornatis, excitavi electricitatem. Tetigit ignis electricus ſuppoſita corpora. Repetii electricitatis agitationem per complures dies. Tum vero in metallis et vaſis, quæ ſub tubo metallico icta fuerant electrico igne, præter opinionem deprehendebam partium nitroſarum varie contextarum magnam copiam. Plura adhuc in conclavi, ubi experimenta inſtitueram, vaſa poſita erant, quæ vero electrica materia ex tubo metallico non percuſſerat. In his nullum erat veſtigium nitri. Ex quibus facile conjectu eſt, ex aqua nitri partes electricitate abripi, derivarique in loca, quæ igne electrico feriuntur.

Sub anni præſentis 1748 initium Venetiis literas accipiebam, quæ hanc conjecturam maxime confirmant. Auctor literarum, *Joannes Daniel Gaiſel*, rem narrabat, quæ *Venetiis*, *Bononiæ*, aliiſque in urbibus *Italiæ* doctiſſimorum ſummorumque virorum animos excitavit. Adjecta erat epiſtola *Italica*, typiſque expreſſa *, quam *Jo. Franciſcus Pevati*, vir

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* Lettere ſopra L' Electricitá principalmente per quanto ſpetta alla Medicina. In *Venezia* appreſſo *Simone Occhi*, con Licenza de Superiori 1747.

vir juris ſcientia præſtantiſſimus, reipublicæ *Venetæ* reviſor, et typographicorum inſpector ſupremus, contexuit. In hac epiſtola, quæ de electricitate medica inſcribitur, clariſſimus *Pivati* mirabilitatis plenorum effectuum hiſtoriam Academiæ *Bononienſis* ſecretario *Franciſco Mariæ Zanotti* exponit. Artem vero, qua, quæ tradit, effecta ſunt, ipſe reperit atque adhibuit *Pivati*. Manifeſtum virtutis ſuæ exemplum electricitas in balſamo *Peruviano* edidit. Hic in cylindro vitreo ita incluſus atque abditus latuit, ut, antequam electricitas adhiberetur, per vitrum omni cura obſeptum nihil tranſmitteret odoratui obvium. Hac in cuſtodia cum eſſet balſamus, ad cylindrum vitreum accedit homo, qui coſtæ alicujus dolore affectus, ſuaſu medici hyſſopum parti morbidæ applicuerat. Fricatur cylindrus, excitatur electricitas, imbuitur eadem ægri corpus, afflictus domum diſcedit, ſomnum capit, ſudorem emittit, balſamique vim diſpergit. Veſtimenta, lectus, cubiculum, odorem balſami ſpirant. Somno recreatus capillos pectit. Hos vero balſami vapor ita penetraverat, ut pecten ſuavi odore inficeretur. Poſtridie ſagaciſſimus *Pivati* hominem bene valentem hujus rei prorſus ignarum eadem, quam pridie ægrotus acceperat, electricitate implet. Hic inſcius conſilii, quo eum electricitatem ſubire juſſerat *Pivati*, relictis hujus ædibus, poſt horam dimidiam, cum in ſodalitio verſaretur, teporem ſentit ſenſim per totum corpus ſe diffundentem. Vigeſcit, et, præter melancholicam corporis ſui temperaturam, hilaritate movetur. Inter quos agit, ſodales odorantur miranturque ſuavitatem certam neſcii, unde exiſtat. Is ipſe, ex quo ſpiritus illi jucundi effugiunt, corporis ſui odorem ſentiſcit,

et

et miratur quid causæ subsit, inscius plane virium, quibus ex vitreo cylindro sapientissimi *Pivati* impletus accesserat.

Portentosæ hujus rei expositione vehementer excitatus certas statim experiri cœpi materias, in quibus electrica virtus cieri potest. Facta pericula veritatem comprobarunt. Contritum sulphur immittebam in sphæram vitream ita operculatam et oblinitam, ut ex ea super igni versata nihil sulphuris odoratui occurreret. Sphæra refrigerata, adhibebam electricitatem. Protinus sulphurei vapores prodibant, qui electricitatis continuatione ita referciebant aerem, ut ad decem pluriumque pedum distantiam nares ferirent. Amicum quendam, in re electrica apprime versatum, professorem philosophiæ extraordinarium *Hauboldum*, aliosque homines suscepti negotii partim rudes, partim conscios advocabam testes et judices, qui vero graveolentia sulphuris statim abigebantur. Ego vero aliquanto diutius atmosphæræ sulphuratæ immoratus foetore abundabam. Vestes, corpus, et ipse spiritus oris foetebant. Imo die post sulphur olebam: quin, instituta repetitione, cum me convenisset virium sulphurearum peritus, fiebat, ut tertio die signa inflammati sanguinis in ore conspicerentur. Posthæc molitus sum effectionem odoris jucundi. Replevi sphæram vitream cinnamomo. In quo eadem, qua dixi, cura et ratione circumsepto cum electricitas vires suas experiretur: adstantibus halitus cinnamomei occurrebant pauco tempore ita augescentes, ut per universum conclave dissipati intrantium nares statim occuparent. Ad posterum diem conclave aromaticum odorem servavit. Pari successu balsamum *Peruvianum* tentavi. Amicus nominatus, cujus

cujus testimonio carere nolebam, postquam electricitatis adminiculo vim balsami conceperat, tantum odorem spiravit, ut per plateas digressus cœnatum convivarum naribus negotium facessens interrogatus fuerit sæpius, quid odoraminis haberet. Ego postridie, cum potum Thée gustarem, insolita saporis suavitate afficiebar. Dimota suspicione, qua ductus de admisto aromate percontabar, ex reliquis in ore spiritibus balsaminis imbui saporem cognoscebam. Paucis interjectis diebus tentamen redorsi a vitrea sphæra, in qua balsamus *Peruvianus* conclusus nihil omnino exspirabat, catenam in conclavi extensam per fenestram libero aeri commisimus, ex eoque in conclave a priori prorsus sejunctum produximus. In hoc suspensam ex filis sericis catenam in manus tradebamus homini extenso reti serico superstanti, nostrique instituti plane rudi. Commotis aliquamdiu electricitatis viribus homo tenens catenam interrogatus, numquid subodoraretur, nares intendens annuebat; quo vero nomine nuncuparet odorem, nescire se profitebatur. Per horæ quadrantem continuatis commotionibus electricis, ita olebat conclave hoc, ut homo, cui de balsamo nostro nihil constabat, dulci odore, qualis in balsamo certo reperiretur, nares suas impleri diceret. Ex somno, quem in domo ab isto conclavi longe dissita cœpit, mane surrexit admodum alacris, et ex potu Thée gustato saporem solito gratiorem percepit.

His pensitatis, non dubito, quin, si quid auxilii petere posse medicinam ex vi electrica existimem, opinione duci videar probabili. Quæ ab arte salutari expectari potest, utilitatis duo præcipue sunt capita. Aut enim res nocivæ, quæ sanguini cæterisque corporis

poris humoribus immixtæ ſanitati officiunt, ſegregandæ ſunt atque expellendæ: aut ſalubres, quæ ad tuendam firmandamque valetudinem proſunt, inferendæ et diſtribuendæ. In utroque genere adjutrix adhiberi poteſt electricitas. Hæc enim ſimul ac corpus humanum tangit, e momento hoc ita permeat, ut nullus in eo locus ſit in quo non verſetur. Quod compluribus indubiiſque experimentis patet. Vi autem tanta pervadit, ut, quod in corporibus volatile effici poteſt, hoc non ſolum ſolvat, ſed etiam diſſipet et ſecum abripiat. Nihil igitur eſt, quod dubitemus, ſanguinem quocum electricitas communicatur, in partes minutiores diſcerpi, earum quam plurimas a maſſa ſanguinea divelli, et brevi tempore in aerem diſpelli. Non retundit vim electricam ſanguinis tenacitas, non cohibet avulſionem venarum firmitas, non reprimit pinguedo. Vitri cohærentia, licet multo firmior ſit venarum et carnis et cutis contextu, tamen impedire non poteſt, quominus ſpiritus et aromata in partes ſolvantur per vitri anguſtias avolantes. Satis igitur cauſæ habere videmur exiſtimandi, electricitate effici poſſe, ut ex ſanguine pariter ac reliquo corpore materiæ certæ ſecernantur.

Sanguinem et humores corporis per electricitatem valde agitari, reſolvi et attenuari patet. Novi enim feminam, cui ſtatim catamenia profluunt, cum electricitatem ſubeat. Medicus quidam nomine *Thebeſius* ante paucos dies mihi ſcripſit *Hirſchbergio* in *Sileſia*, ſibi nuper, ſi electricitatem patiatur, hæmorrhagiam narium ſemper ſupervenire.

Non autem disjungendi tantum et expellendi vim habet electricitas, ſed potentiſſima etiam eſt ad locupletandum ſanguinem viribus, quæ in plantis et mi-

 neralibus

neralibus continentur. Quod ex iis, quæ de ſulphure, cinnamomo et balſamo *Peruviano* expoſui, ſatis intelligi arbitror. Electrica via nutriendi ſanguinem cum in hoc, quod ſanguini ſine ſtomachi ope alimenta ſuggerit, a conſueta medicis ratione differt, tum halitibus, qui, quod per vitrum migrârunt, ſubtilitate et puritate excellunt, ſuccum vitalem ditat. Medicamenta, quæ ore accepta in ſtomachum ingeruntur, antequam cum ſanguine miſceri poſſunt, per multas longaſque vias errare, in iiſque immutari debent. Sed qui alma electricitate aguntur, ſpiritus ſine his anfractibus ſanguinem influunt. Interdum aliqua corporis pars ex eo laborat, quod viæ, per quas ſanguis aut alius liquor affluere debet, adeo obſtructæ ſunt, ut, quæ adhibentur, remedia ad eas aperiendas aut nihil valeant, aut longo tempore opus habeant. Quam vero partem afflatu contingunt electricitate provecta ſpiramina, hanc perniciter aperiunt penituſque penetrant.

Medicinæ igitur artiſque electricæ conjunctione effici poſſe exiſtimo novas feliceſque morborum curationes, quarum exempla inſignia edidit prudentiſſimus *Pivati* ſcientis exercitatique medici conſilio uſus. Impeditum obſtructumque fluxum ſanguinis in femina aliqua ſtatim reſtituit, ita tractatis remediis, quæ adhiberi ſolent, ut eorum vires ex cylindris vitreis, in quibus occluſa fuere, electricitatis adminiculo corpus ægrotantis attigerint. *Pivati* curam imploravit nobilis juvenis, ex collecti corruptique in pede humoris abundantia adeo miſere affectus, ut morbus operam medicorum omnem eluderet. *Pivati* vitreum cylindrum rebus congruis impletum inſtructumque ope machinæ electricæ fricat; electricitatem

citatem in ægrotum derivat; ex loco, in quo morbus residebat, scintillas electricas elicit, idque per aliquot minuta prima continuat. Nox sequitur, ægrotus se somno tradit, quiete fruitur dolore mitigato, evigilat, prope talum parvum sed rubrum tuberculum videt, nil nisi pruritum sentit frigido quasi humore per interiorem pedem fluente. Per octiduum singulis noctibus peringenti sudore maduit, et, exacto hoc tempore restitutus bene valuit. Post Episcopus *Sebenecensis*, *Donadoni*, cum medico suo et nonnullis amicis ad *Pivati* accessit. Præsul annos septuaginta quinque eo tempore natus manuum pariter ac pedum doloribus ex longa annorum serie laborabat. Chiragra digitos ita incurvaverat, ut extendi flectique vix possent. Podagra eum ita afflixerat, ut genua flectere ægre valeret. Adeo miser erat, ut noctu somnum capturus a servis ex sella prope lectum collocata in hunc transponendus esset, pedibus ante leniter repositis. Æger senex petiit a *Pivati*, ut experiretur, quid electricitas in corpore suo valeret. Modus medendi fuit sequens. Cylindrus vitreus complexus materias viribus discussoriis instructas ita agitatur, ut virtus electrica prodeat in Præsulem. Hic derepente commotiones insolitas in digitis sentit. Actio electricitatis per duo minuta prima continuatur. Opinione citius Præsul libere alacriterque utramque manum dilatat et contrahit, unum ex comitibus manu vehementer apprehendit, surgit, ambulat, manum manui allidit, sellam occupat, pedem unum supplodit, et vires suas miratur nescius quasi vigiletne an somniet. Abit ex conclavi, sine manu adjutrice scalam descendit, et more valentis juvenis in lembum se confert. Paulo post

Pivati matronam fexagenariam fimili modo liberavit arthritide, qua fex menfes vexata fuerat. In magno tumore fuerunt digiti continuo trementes, et brachium unum convulfionibus agitatum eft. Sed poft duo minuta prima, quam vires electricitatis experta erat, tremor digitorum defiit. Poftero die tumor ita decrevit, ut chirothecas induere, et officio manuum fungi potuerit matrona.

Hæc adeo clara funt, ut nullus videatur locus dubitandi de auxilio, quod medicinam fibi ex electricitate comparare poffe cenfeo. Qua in fententia animum meum confirmat fuffragium, quo judicii plenus et in re medica verfatiffimus *Morgagni*, in academia *Patavina* anatomiæ profeffor, explicatum fibi a *Pivati* negotium egregie comprobavit, eumque, fubminiftratis confiliis, impenfe cohortatus eft ad rem medicinæ accommodatam generique humano fructuofiffimam novis fubinde laboribus perficiendam.

XIX. *A Letter from Mr.* Henry Baker *F. R. S, to the* Prefident, *concerning feveral* Medical Experiments *of* Electricity.

S I R,

Read March 13. 1748.

THOUGH perhaps as many curious and well-contrived Experiments have been made in *England* as in all the other Parts of *Europe*, to difcover the general Laws and Properties of *Electricity*; we have not hitherto attended to the Effects that may be thereby produced in the Bodies of living Animals, any further than

to

to assure ourselves they may be killed thereby; a Supposition that Diseases may be cured by means of this Power, having met with so little Countenance amongst us, that very few Trials have been made, to ascertain what, in distemper'd Cases, it can or cannot perform. *Foreigners*, on the contrary, seem fond of believing, that the subtil electric Fluid (be it Fire, Æther, or whatever else) which can pervade all Bodies, and (being accumulated) even kill an Animal, in certain Circumstances, and by certain Methods of Application, may, possibly, in other Circumstances, and applied in different Degrees, and by different Methods, so operate on the Fluids or Solids, and perhaps on both, that very beneficial and salutary Effects * may result therefrom.

With this View the Abbè *Nollet* made several Experiments on living Birds, Kittens, and human Bodies; and if we may give Credit to the Accounts thereof communicated to us, he found, in every Trial, that *Perspiration* was so considerably promoted thereby, as to cause a very sensible Difference between the Weight of such Animals as had been electrified, and others of the same Kind that were treated exactly alike in every respect besides: Whence he naturally concludes, that, in Cases where it is necessary to quicken the Circulation of the Fluids, and throw off a greater Quantity of the perspirable Matter, *Electricity* must be greatly useful.

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* As is suggested by Dr. *Mortimer* in these *Transact.* n. 476, p. 479. C. M.

" The Philofophers in *Italy* and *Germany* have applied their Induftry to difcover by Experiment, how far Electricity may, fimply and in itfelf, be of Service in feveral Difeafes, and likewife how far it may conduce towards conveying the more fubtile and active Effluvia of ufeful Medicines, either into the whole Body, or into fome diftempered Part.——Mr. *Watfon* read, laft *Thurfday*, before the *Royal Society*, an Abftract of the preceding Paper, fent to Dr. *Mortimer* from *Leipfic*, by Profeffor *Winckler*, of feveral Experiments to this Purpofe, made at *Venice* by M. *Pivati*, and repeated afterwards by himfelf at *Leipfic* with the fame Succefs. He gives Inftances of faturating, by Electrification, with the *Effluvia* of Balfam of *Peru*, and of Sulphur, fo as to produce very remarkable Effects; and of taking a Fit of the Gout away intirely, by conveying into the Part afflicted the fanative *Effluvia* of warm and difcutient Drugs.

My ingenious Friend Dr. *Jofeph Bruni*, one of the principal Phyficians at *Turin*, and Fellow of our *Royal Society*, has likewife fent to me an Account, lately received by him, of Experiments made at *Rome*, and at *Bologna*; which I now, Sir, lay before you, in order to fhew what Attempts to the fame Purpofe have been made in different Countries, and by different People.——The Doctor informs me, that at *Turin* they have repeated, with great Succefs, the electrical Experiments made in *England*, whereof I had fent him printed Accounts; that People all over *Italy* are bufily at Work making electrical Experiments; and that, at *Bologna*, the electrical Power has been applied to the Cure

of

of Diſeaſes. He then gives me a Tranſcript of an Account ſent him from thence in the *French* Language, which, tranſlated, is as follows.

A Man, who had been for a whole Twelvemonth deaf of one Ear, with a continual Noiſe in it like the Running of Water, attended with moſt violent Pain whenever he lay with that Ear uppermoſt, coming to Dr. *Verati* for Advice, the Doctor electrified him, bringing out Abundance of fiery Sparks around the diſtemper'd Ear; which, in about five Minutes that the Electrification was continued, became as red as if a bliſtering Plaiſter had been applied to it. But the Redneſs diſappeared in a few Minutes after, the Patient paſſed the Night with leſs Pain and Noiſe, and was perfectly cured of his Diſorder.

A Footman belonging to the ſaid Doctor, being taken ſuddenly ill of a violent Pain in the Head, which continued many Hours, he was thereupon electrified, the Doctor cauſing the Sparks of Fire to iſſue from the Temple wherein the Pain was felt. The Part appeared red, the Pain abated; in three Hours it was intirely gone, and has never returned ſince.

A Woman that nurſed one of the Doctor's Children, having had a moſt grievous Diſorder in her Eyes for ſome Months, with a continual Running of Water from one of them, and a conſtant Pain over the Eye-lid, came to the Doctor for Advice; who immediately electrified her, bringing out the fiery

fiery Sparks about the Eye and Eye-lid, whereby the Eye appeared very much blood-ſhot; but that went off in 7 or 8 Minutes. The Woman felt leſs Pain the following Night, and opened her Eye in the Morning more eaſily, and without being obliged to wipe it, as ſhe did before: The watry Humour and Pain were much diminiſhed; and the Doctor hoped, that, by repeating the Operation twice more, he ſhould be able to cure her quite.

Dr. *Bruni* gives me next his Information from *Rome*; which is, that a Gentleman there cover'd the internal Surface of a Cylinder of Glaſs (which ſome uſe inſtead of a Globe) with a purgative Medicine; and that a Man, electrified therewith, found on the Spot the ſame Effects as if he had ſwallowed the Medicine. He then recommends to us in *England* to try how far the electric Power may be of Service in Diſtempers.

Theſe Caſes, Sir, and particularly the laſt, as it may to ſome appear extravagant and whimſical, I ſhould have been cautious of bringing before the *Royal Society*, had you not judged it proper they ſhould be added to thoſe ſimilar Accounts from other Places which were read to us laſt Meeting. I think neither myſelf nor Dr. *Bruni* anſwerable for the Truth of theſe Facts, as we relate no more than what we have received. In Truth, all the *Phænomena* in Electricity are ſo wonderful, that it is ſcarcely prudent to deny the Poſſibility of any Accounts concerning it, till we have made Experiments carefully ourſelves. We are *very ſure* it is poſſible to

render

render a living Body replete with electrical *Effluvia*, or to transmit and send such *Effluvia* through a living Body, in a Stream, as long as we think proper: We are *not sure* that it is impossible for these *Effluvia* to convey with them into that living Body the most subtile and active *Effluvia* of other Substances; and if they can do so, the Effects suggested are not wholly improbable; for several Experiments have proved, that a very minute Quantity of Medicine, transfused directly into the Blood, and circulating Fluids, will have the same Effect as a large Dose thereof taken into the Stomach. Therefore even this last Case, romantic as it may seem, should not be absolutely condemned without a fair Tryal; since we all, I believe, remember the Time, when those *Phænomena* in Electricity, which are now the most common and familiar to us, would have been thought deserving as little Credit, as the Case under Consideration may seem to do, had Accounts of them been sentus from *Rome*, *Venice*, or *Bologna*, and had wenever experienced them ourselves.

I am proud to seize every Occasion to assure you with what great Respect I am,

SIR,

Strand, March 28, 1748.

Your most faithful and obedient humble Servant,

Henry Baker.

ERRATA.

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Numb. 487.

PHILOSOPHICAL TRANSACTIONS.

For the Months of *April*, *May*, and Part of *June*, 1748.

I. *A Propoſal for Checking in ſome Degree the* Progreſs *of* Fires; *by the Rev.* Stephen Hales, *D. D. & F. R. S.*

Read April 21. 1748.

THE late deſtructive Fire in *Cornhill* bringing to my Mind what I thought a probable Means to check, in ſome degree, the ſpeedy Progreſs of Fires (which, if it could be effected, would be of great Importance I made the following Experiment, in order to form a more certain Judgment of the Matter; *viz.*

I placed on two Garden-Pots a dry Fir-Board, which was half an Inch thick, and nine Inches broad; and cover'd nine Inches Length and Breadth of it with an Inch Depth of damp Garden-Earth; fencing this Earth on each Side with two Courſe of Bricks, in order to make a Fire-place to contain the Wood-Fuel and live Coals; which were frequently blown with Bellows, in order to keep the Fire to a vigorous Heat: This was done for two Hours Continuance, before the Fir-Board was burnt thro'; when there was only a weak lambent Flame at the under Part of the Board; for it could not flame out for want of proper Fuel; becauſe the Subſtance of the Board was reduced to a brittle Charcoal, by the Heat of the Inch-Depth of Earth which lay on it, which hinder'd the burning Board from flaming. And it was obſervable, that the Edges of the Board burnt only with a live Coal like a Match; being hinder'd from flaming, by the Earth which lay on the Board.

May it not hence be reaſonably inferr'd, that, when a Houſe is on Fire, it may be a probable Means conſiderably to retard the Progreſs of the Fire, to cover with Earth the Floors of the adjoining and more diſtant Houſes, which ſtand in the Courſe of the Progreſs of the Flames?

The thicker the Earth is laid, ſo much the better: But if Time will not permit to lay it more than an Inch thick, then ſuppoſing 27 Men to carry each a cubic Foot of Earth, which will be a cubic Yard of Earth; then that cubic Yard of Earth will cover 36 ſquare Yards of Flooring; which repeated ſeveral times, would ſoon cover all the Floors of a Houſe. And as the Fire probably mounts with great Fierceneſs up the Stair-Caſe, it will be well to lay much Earth on the Stairs; which will help to give ſome Check, eſpecially as the Earth on the Floor and Stairs may be wetted by the Fire-Engine; which Moiſture will be much the longer retained by means of the Earth; whereas Water, when not thus retained, ſoon glides away.

And as Fires often catch from Houſe to Houſe at their upper Parts, an upper Floor cover'd with Earth, with the Rafters burning on it, will be longer in burning to ſuch a degree as to fall on the next Floor, ſo, when fallen there, it will alſo be the longer in burning, and will flame the leſs, on account of the Earth on that next Floor; and, conſequently, will not be ſo apt to fire the next Houſe, as in the common Caſe of Floors without Earth, which muſt needs therefore burn the more fiercely.

Earth may be had either in back Yards, or Cellars, or Streets.

These Hints, from one who never saw a House on fire, will, 'tis hoped, be farther improved by those who have more Experience and Skill in these Affairs.

II. *Some Observations, made during the last three Years, of the Quantity of the* Variation *of the* Magnetic *Horizontal* Needle *to the Westward; by Mr.* Geo. Graham, *F.R.S. at his House in* Fleetstreet, London.

Read Apr. 21. 1748.

				° ′
1745	*March*	26	- - -	17 = 0
		29	- - -	17 = 0
	March	18	- - -	17 = 10
		21	- - -	17 = 10
	April	22	- - -	17 = 15
	May	4	- - -	17 = 18
		14	- - -	17 = 20
		16	- - -	17 = 15
	Dec.	18	- - -	17 = 25 +
	Febr.	24	- - -	17 = 30
1747	*Dec.*	19	- - -	17 = 40
	Jan.	4	- - -	17 = 40 —

The Inclination of the dipping Needle has been during the same time about 73½ Degrees.

 N. B.

N. B. As the Variation of the Needle at London *has not been regularly published from time to time in the* Philosophical Transactions: *It may not be improper to take notice here, that according to the best Observations extant, and which were made by Persons of great Skill and Exactness, the Needle at* London *declined to the* Eastward 11° 15′ *in the Year* 1580. *In* 1657 *there was no Variation, the Needle then pointing due North. In* 1672 *the Variation was observed by the late Dr.* Halley 2° 30′ *towards the* West, *and in* 1692 6° 0′. *And towards the Beginning of the Year* 1723, *it was found by* Mr. Graham, *from the Medium of a vast Number of Observations, to be then* 14° 17′ *the same Way. So that, during the Course of* 167 *Years elapsed since the Year* 1580, *to the End of the last Year* 1747, *the magnetic Needle at* London *has moved to the* Westward, 28° 55′. See before N°. 148, and N°. 383 of the *Philosophical Transactions.*

III.

III. *A Letter from the Right Hon.* John *Earl of* Orrery *to* Martin Folkes *Esquire, Pr. R. S. inclosing an Account of the* Cornel-Catterpillar, *contained in a Letter from the Reverend Mr.* Philip Skelton *to His* Lordship.

SIR,

Read April 21. 1747.

THE great Honour which I have received from the *Royal Society*, makes me very ambitious of expressing my Sense of it, in some manner that may at least be a Mark of my Gratitude, however unworthy of their Attention. Inclosed in another Packet I send you a Letter I have received from the Reverend Mr. *Philip Skelton*, who, at my Request, has drawn up the Account of a sort of Caterpillars, that appeared very numerous in these Parts some Years ago. He has likewise pasted on the first Leaf a Piece of their Web *. He is my Neighbour here, and a Gentleman of Sense and Learning. The Letter is so full and explicit, that I need say nothing on the Subject: Unless hereafter you are desirous to know further Particulars; and I hope you are convinced, that I shall be always ready to obey your Commands. I am, Sir, with the greatest Esteem, and the utmost Respect,

Caledon, April 6. 1748.

Your most obliged and obedient humble Servant,

ORRERY.

The

* To be seen in the *Musæum* of the *Royal Society*.

The Rev. Mr. Skelton's *Letter to the Earl of* Orrery.

Monaghan, March 29, 1748.

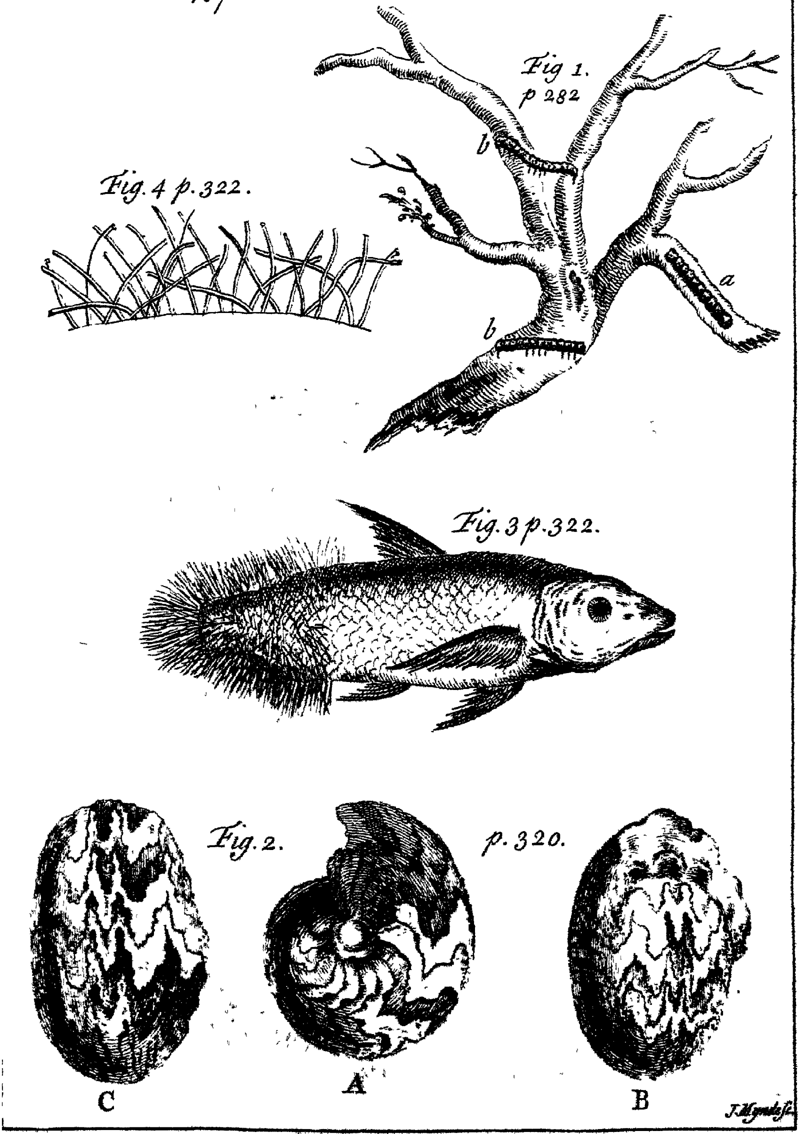
MY LORD,

PURSUANT to my promiſe, I ſend you an account of the Cornel-Caterpillar, the web it produces, the flie into which it is changed, and a ſmall ſample of its work.

In the beginning of *May* 1737, the warmeſt ſeaſon that any body now alive remembers to have felt, the Cornel-trees, of which we have a good number about this place, appeared almoſt cover'd with ſmall Caterpillars of the ſize and ſhape in TAB. I. *Fig.* 1. and in general of a duſkiſh green, reſembling in colour the bark of the tree, altho' a few, conſiderably larger than the reſt, were yellow. Theſe worms were employed partly in feeding on the leaves of the Cornel, which was their only Nouriſhment, and partly in crawling (with a very ſwift motion for a worm) over the bark of the tree. As they crawled, they left each a fine thread, ſcarcely viſible to the naked eye, ſticking to the bark. Theſe threads, being almoſt infinitely multiplied by the inconceivable number of worms employed in the work, formed the web, in which the threads are not interwoven, but cohere by ſome roughneſs or glutinous quality.

By the end of *May* there was not a leaf to be ſeen on any of the Cornels, excepting a few, reſerved for a very curious purpoſe, which I ſhall have occaſion

Philos. Trans. No. 487.



occaſion to mention preſently. But the worms, in the room of the green cloathing they robbed thoſe trees of, gave them one of white, ſo entire, that it covered the whole bark, from the ground to the points of the ſlendereſt twigs, and of ſo pure and gloſſy a colour, that the whole tree ſhewed in the ſun as if it were caſed in burniſhed ſilver. The web was ſo ſtrong, that if one diſengaged it from the tree, near the root, one might have ſtripped it from the trunk, the branches, and the twigs, at one pull. As ſoon as the worms had covered all the Cornel-trees, they removed from thence, and covered all the Aſh, Beech, Lime, Crab-trees, and even weeds, that grew near them, with the ſame, but a thinner, kind of workmanſhip.

Perhaps, my Lord, you will deſire to know how they travelled from one tree to another. Many of them crawled along the ground, and over every thing in the way, ſtill leaving a thread behind, and diſpatching a part of their buſineſs as they went to a more convenient ſurface to finiſh the reſt on. But I really imagined ſome of them took an eaſier and more ingenious way. I found many of them hanging by their own threads from the moſt extended branches of the tree. While they were in this ſituation, a gentle puff of wind might, by exciting a pendulous motion, waft them to the next tree. This ſeems to be the method, by which thoſe very minute ſpiders, whoſe threads are made viſible by the moiſture adhering to them in a foggy morning, tranſport themſelves from one buſh to another, altho' deſtitute of wings, ſometimes acroſs narrow paths, and even rivulets.

As the worms, neither while they were working it, nor afterwards, made any uſe of the web thus left on the bark of the trees, I take it for granted, they wrought for no other purpoſe than to rid themſelves of that glutinous maſs, out of which it was ſpun, and which, nature producing it that ſeaſon in greater abundance than was neceſſary for the wrapping and ſtowing the worm in its *Nympha* ſtate, prompted the creature to work off the redundancy the beſt way it could. The method it made uſe of for this purpoſe was very well judged. It faſten'd its thread to ſome little eminence on the bark; and chuſing, for the greater convenience of crawling, that even ſurface, kept continually in a briſk motion, till the troubleſome ſuperfluity of its burthen was diſcharged. I can but gueſs at its reaſon for removing from its own native tree, to ſpin abroad upon the neighbouring ones. Perhaps it found the web too bright for its eyes, or the threads, already layed, might have ſtuck to its feet; for your Lordſhip may obſerve that the web is very apt to ſtick to the fingers, when it is touched.

About the beginning of *June* the worms retired to reſt. Their manner of preparing for, and executing this, was very ingenious and curious. Some of them choſe the under ſides of the branches, juſt where they ſpring from the trunk, that they might be the better defended from the water, which in a ſhower, flowing down the bark of the tree, is parted by the branches, and ſent off on each ſide.

Here they drew their threads acroſs the angle, made by the trunk and branch, and croſſing thoſe again with other threads in a great variety of directions

rections, they afterwards formed a strong tegument on the outside. Within this they placed themselves lengthways among the threads, and rolling their bodies round, spun themselves into little hamocks of their own web, while in the mean time they shrunk into half their former length. Those hamocks, being suspended by the transverse threads, did not press each other in the least. That they might take up the less room, they lay parallel to one another, and in the most convenient order imaginable.

Others, still more ingenious than these, fastened their threads to the edges of certain leaves, which, no doubt of it, they had saved from their stomachs for this very purpose; and with that slender cordage pulling in the extremities of the leaves, drew them into a kind of purse, in the inside of which they formed the same kind of work, and laid themselves up in the same manner as above. By this method they saved themselves a labour, which the rest were at the expence of; for the leaf served them very well for an outward defence against the weather, and a place to fix their transverse threads to. It is probable they laid themselves up in great numbers together, not only because many were necessary to the work of providing a common covering, but also to keep one another warm, while nature was preparing for the great change, and also to confine some subtil vapour, issuing from their bodies, which might have been conducive to their revivifcence, and which had been easily dissipated, had they not lain close, and caught it from one another.

Between the worm, thus laid up, and the hamock, in which it was enclofed, a tough and pliant fhell, of a dark-brown colour, was found. This I take to have been formed by the perfpiration, or rather by fome glutinous ftuff, forced through the pores of the infect, while it was contracting itfelf, which being ftopped by the clofe texture of the hamock, confolidated, and formed an interior covering for this delicate creature. As the worms themfelves were of a pretty dark colour, this fuperficial tincture feems to have been in a great meafure purged off into the fhell.

For after the worms had continued in this ftate during the whole month of *June*, whether they gnawed their way through the ends of their fhells and hamocks, or that exit was prepared for them by fome corrofive matter ouzing from their mouths, I know not, but they came out almoft all in the fpace of one morning, the moft beautiful flie or moth that my eyes ever beheld. Its fhape was extremely elegant; its head, upper wings, body, legs, and *antennæ*, were of the pureft white, and glitter'd as if they were frofted with fome fhining kind of fubftance. I rubbed fome of this off, and upon viewing it thro' an ordinary microfcope, it appeared like the points of very minute feathers, or like fmall cones of polifhed filver. The upper wings were regularly ftudded with fmall, round, black fpots, and extended themfelves from its head fomewhat beyond its tail. The under wings, which were a little fhorter, were of a duskifh colour, and prettily fringed at the extremities.

This

This beautiful and ſurpriſing work of nature ſeemed, after its reſurrection, to have no dependence on material food. The Cornel had recovered a new ſet of leaves by the time the flie appeared; but it never touched them; and thoſe that came out in my room, lived as long there, as the reſt which enjoyed the open air, and the tree on which they were bred. If they did feed, it muſt have been on ſome other adventurer of the air, too minute to be viſible to our eyes. Thoſe that were confined to my room, diſcharged a ſmall drop of brown liquor, in which I ſuppoſe their eggs were contained; but as they were not depoſited in a proper receptacle, they did not produce worms the next year. As the tree is the peculiar habitation of the worm, and ſupplies it with its only food, ſo it is certainly the only nurſe of its egg. It is likely the eggs were either inſerted into the ſmall crevices of the bark, or diſcharged into the little apertures, where buds are to ſpring the following ſeaſon. In this ſituation, they might be moſt conveniently nouriſhed by the return of that genial juice, or ſpirit, with which the Cornel is naturally fitted to cheriſh and raiſe them into life. The flies ſeemed to be of a moſt delicate conſtitution in reſpect to heat and cold. The former they could bear with difficulty; the latter, not at all. Hardly any of them ſurvived the firſt of *Auguſt*. They loved reſt, and did not care to flutter much about. While they were yet in their *Nympha* ſtate, I brought great Lumps of them to my room, and thoſe, which happened to be bruiſed in pulling them from the trees, produced flies, diſtorted either in the wings or other parts; but this

Diſtortion generally wore off, in a little time, and the pretty creature recover'd its own natural ſymmetry of ſhape.

In the beginning of *May* 1738, they began to work again in prodigious numbers, and having covered ſome trees, were ſtopped, and moſt of them deſtroy'd by the foul weather that followed. Their web alſo was ſmutted and diſcolour'd. I ſend your Lordſhip a piece of each year's produce. The whiteſt is that of 1737, the other of 1738.

In 1739 they appeared in ſmall numbers, and much ſhrunk in their Size, and wrought only ſufficient covering for themſelves.

They appeared again in the Year 1740; but it was plain the great froſt had deſtroyed moſt of their eggs, and checked the growth of thoſe that eſcaped; for there were very few of them to be ſeen, and twelve of them were not larger than one in *May* 1737.

Every year produces more or leſs of them, with ſome ſmall variation, as to the number and ſize.

The place where our Cornel-trees ſtand, is ſurrounded with ſteep hills, and cloſely ſhelter'd with a very thick plantation. This was probably no inconſiderable help to the prodigious encreaſe of this puny reptile. I verily believe both an unuſual warmth of air, and a deep ſhade, were equally neceſſary to it; for I obſerved, that thoſe Cornels, which ſtood more expoſed to the cool air and the ſun, abounded leſs with worms than the reſt.

I have been ſcrupulouſly exact, my Lord, in relating the above particulars, which I did not truſt to

to my memory, but reduced to writing immediately after I had finiſhed my obſervations.

This curious *phænomenon* naturally leads one to enquire, how theſe creatures came to breed on the Cornel-trees, and what occaſioned the prodigious encreaſe of them at that time. Here fact gives us up to conjectures. I hope however that mine will not ſeem to your Lordſhip altogether unſatisfactory, but rather help to clear up thoſe difficulties, and at the ſame time carry our eyes a little farther into nature, than merely to what concerns this ſpecies of inſects.

There is not an animal, nor a vegetable, that may not be conſidered as a little world, in reſpect to the habitation and nouriſhment it affords to certain inſects peculiar to itſelf. The ſcheme of life begins in vegetation; and wherever on the earth, or in the water, nature is able to produce vegetables, ſhe always obliges them to pay for their elemental nouriſhment to certain inſects, animals, or fiſhes, which ſhe billets on them. Theſe again are forced to refund to others, to diet and lodge, each of them, a ſet of living creatures, aſſigned to them by the univerſal ſcheme of nature.

This traffique of life, this juſt community in nature, which ſuffers nothing to ſubſiſt merely for itſelf, is found not only every-where on the face of the earth, but alſo in all lakes, pools, rivers, and in the ocean. By microſcopes we diſcover a prodigious variety of little creatures, all feeding either on the floating vegetables, which that element produces in a ſtate of ſtagnation, or on one another. As to the ſea in particular, we know only what happens

happens about the ſhores, where we ſee vegetables of various kinds, on which a like variety of inſects are bred and nouriſhed. Theſe, together with a prodigious number of others, bred in the mud, become the prey of the ſmaller kind of fiſhes, and they again of the greater. That this ſcheme of nature, in ſupporting life by death, found every-where elſe, dives into the depths of the ocean, may appear probable from the wiſe frugality of nature, which hath an uſeful end in every thing, and beſides rejoices in filling the world with life and motion; and alſo from the wonderful kinds of fiſhes, which are now-and-then waſhed up by violent ſtorms from the deep waters, or happen to purſue their prey, from the low lands of the ocean, to the higher grounds at the ſhores.

Franciſcus Redi, in his curious treatiſe concerning the generation of inſects, hath not only refuted the notion of equivocal generation, but alſo hath ſhewn us, that each animal and vegetable hath its own peculiar inſects to maintain; and *Eleazar Albin*, in his collection of various caterpillars, and the butterflies, into which they are transformed, hath given us a beautiful demonſtration, from above an hundred inſtances, that each ſpecies hath its own proper plant, to which it is by nature peculiarly adapted, and on which only it can feed, or live for any conſiderable time.

Now the Cornel, my Lord, is the plant, on which alone the worms, we have been ſpeaking of, can be propagated and fed. The ſpecific qualities, with which the juices of this tree are impregnated, fit it

for

for the propagation and ſupport of this its native inſect. If theſe peculiar and diſtinguiſhing qualities reſide, as the chymiſts ſay, in the eſſential oil of the plant, it will follow, that this, as well as other inſects, ſubſiſting on vegetables, are by ſome means or other qualified to extract, in a nicer manner than any chymiſt can do, the eſſential oil of their reſpective plants, nothing elſe therein being of a nature ſufficiently peculiar either to aſſiſt the propagation, or ſupply the nouriſhment, of the inſect.

As to the difficulty, how this plant came to receive the eggs of this flie, it is as great in reſpect to the propagation of any other inſect on its peculiar plant. The flies of every plant have continual acceſs to thoſe plants, and no doubt are prompted by the ſight, ſmell, or other qualities of their native vegetable, which are congenial to them, to propagate their kind upon them. As this act is probably attended with ſome degree of pleaſure, it keeps them continually buſy in the work of impregnating their proper plant. Hence it comes, that before the younger plants are removed to a diſtance from thoſe that are more fully grown, they receive ſufficient colonies from others, already peopled, which they extend again to their ſuccours, the flies each year impregnating all the plants within their reach. Whether the thing happens in this manner, or that the eggs of inſects ſo ſmall are minute enough to be carried through the air, and ſo diſperſed everywhere, it is nevertheleſs a fact, that no vegetable is found without its inſects, tho' propagated by the ſeed.

As

As to the extraordinary increaſe of this inſect in *May* 1737, the ſucceſſion of ſeven or eight mild winters, which preceded that ſeaſon, might, by preſerving their eggs, give occaſion thereto. As they are one of the earlieſt kinds, the exceſſively warm *May* that year ſo effectually hatched their eggs, that they all came to perfection: Whereas the more ordinary worms and flies, that make a later appearance, meeting with the ſharp eaſterly winds that happened that ſummer to blow during the months of *July* and *Auguſt*, were in a good meaſure deſtroyed; otherwiſe it is poſſible they too might have had an extraordinary increaſe.

However I own, my Lord, this reaſon hath its objections, and doth not fully ſatisfy me. There is ſcarcely a year that is not remarkable for ſome one kind of inſects or flies, when no colourable reaſon can be aſſigned for it from any known temperament of the year, which might not as well favour a great increaſe of any other ſpecies. Inſects, as well as fevers, are epidemical, and probably depend as much on a certain occult conſtitution of the air, water, or earth. Nay, it is an opinion received by ſome, that all peſtilential diſorders are nothing elſe than prodigious flights of inviſible flies, of which each ſort, according as the conſtitution of the year aſſiſts it, takes its turn to multiply from worms proportionably little, bred in putrid carcaſes, eſpecially after great battles, and being raiſed from thence into the air, are wafted not only from one body to another, but even to diſtant countries. *Sydenham*, and, if I miſtake not, others, have obſerved, that the ſeaſons immediately preceding thoſe

in

in which the plague raged, abounded unusually with all sorts of flies; which shews at least, that the constitution of the air doth at those times greatly favour the production of such creatures. This conjecture will seem the more probable, as the usual preservatives against infection, namely vinegar, tobacco, rue, wormwood, *&c.* are endued with very acrid and pungent particles, with which perhaps they sting and kill the invisible flies before they can lay their eggs, and by these means preserve us from contagion.

Be this however as it will, it is certain there is such a constitution as we are here speaking of, in respect both to distempers and insects. But whence this proceeds, whether from the sun alone, or from the joint influence of other neighbouring planets, or the transudations of mineral vapours, or fermentations in the soil of the earth; and further, whether this sort of climacteric in the seasons be stationary or casual; I leave better naturalists to judge.

I only insist, that such a constitutional temperament there is, which, running thro' all nature, doth at certain times give more than ordinary energy to the prolific powers of such plants or animals as are of nature similar thereunto.

This plainly appears to us in plants of all kinds, even excluding the consideration of warmer or colder, of drier or moister seasons, which, it is manifest, have only their share in the casualties to which the vegetable world is liable. They frequently bear more blossoms and fruit in a bad, and less in a good season; and what puts the matter beyond all question, that season which is favourable to one

kind of vegetable is prejudicial to another; whereas much heat and moiſture together are equally indulgent to all. This, in my opinion, ſhews that each plant hath a ſpecific vegetation of its own, as well as one common to all, and that the former depends upon ſomewhat elſe than mere warmth and moiſture.

The conſtitution therefore of the year diſpoſes the vegetative ſpirit, whether reſiding in the air, the earth, the water, or in all, to ſupply ſometimes theſe, and ſometimes thoſe vegetables, with a greater or leſs proportion of aliment. By theſe means a greater quantity of that juice, which diſtinguiſhes any one ſpecies of plants from all others, and enables it to give life and food to its peculiar inhabitant, muſt neceſſarily be produced one year, than another; and conſequently the eggs, depoſited in the cavities, or perhaps in the very perſpiratory pores of its bark, muſt be better cheriſhed, and the worm more plentifully fed by the leaves, which in ſuch a year contain a greater abundance of the ſpecific juice, and that more perfectly elaborated.

From hence it may ſeem reaſonable to reſt in this conjecture, till ſomewhat more certain is found out, that the annual conſtitution being more indulgent to the vegetation of one plant than of another, promotes the growth and fertility of this, which is of a ſimilar, and checks the increaſe of that which is of a diſſimilar nature. The plants, thus differently ſupplied, furniſh their reſpective inſects accordingly. Hence again it comes to paſs, that many ſpecies of inſects, having been injured by ſome unknown diſpoſition of the air or earth,

ſeem

ſeem almoſt extinct in one ſeaſon, and ſwarm out again in another, as if there had been a new creation of them. One year, the wall-fruits are devoured by earwigs; another, we are peſter'd every-where, and even in our cloſeſt chambers, with unuſual multitudes of the common flie. One year the waſp predominates; another, the gnat; and a third, the cale-caterpillar. One year, the farmer complains of a worm, hardly known to him before, that deſtroys his corn; and the gardener does the ſame another, in reſpect to an inſect that falls greedily on his ſeeds, as ſoon as they are committed to the ground. The *African* locuſts come ſome years into *Spain* in ſuch ſwarms, that they cover the face of the earth; and when they have devoured the whole herbage of the country, retire again to their own, and do not viſit *Spain* in the like numbers for ſeveral years. Large old orchards are ſome years ſuddenly ſtripped of all their bloſſoms and leaves, by a prodigious increaſe of the apple-tree-worm; and groves of oak have been ſerved in the ſame manner by the caterpillar peculiar to that tree. This muſt needs give a check to the growth of the tree more than equivalent to the great increaſe promiſed at ſuch a time by the extraordinary redundancy of the vegetative ſpirit.

I have now finiſhed what I had to ſay on this ſurpriſing ſubject, at which ſome gentlemen ſtupidly important, may laugh, as at an affair not worthy of ſo much notice, and ſo many words; but I am perſuaded, my Lord *Orrery*, who regards not things by their bulk, but their excellence, will ſee the wiſdom and power of God as glorioully diſplayed in

this little insect, as in the Behemoth, or the Leviathan. It is a flight high enough for the faculties of man to rise by contemplation to a competent knowlege of the meanest work God ever condescended to form. That which was not beneath the Majesty of God to make, can never surely be beneath the dignity of a rational creature to contemplate. I am,

My LORD,

Your Lordship's most obliged, and

most obedient humble Servant,

Philip Skelton.

IV. *The Extract of a Letter from Dr.* James Mounsey, *Physician of the* Czarina's *Army, to* Henry Baker *F. R. S. concerning the* Everlasting Fire *in* Persia.

SIR,

Read April 27. 1748.

AS you inform me any thing relating to the Natural History of *Persia* will prove agreeable, I have some time ago wrote to a couple of Gentlemen, a Physician and a Surgeon, both Men of Learning and Veracity, and my very intimate Friends, who are now with the Ambassador from this Court to *Persia*, and they both have promised to communicate to me whatever they shall meet with remarkable in that Country, and

and you may depend on receiving from me all the Accounts they ſhall pleaſe to ſend.

In the mean while, as the Natural Hiſtory of *Perſia* is but little known, and the Authors of the *Univerſal Hiſtory* have given no true Account of the *everlaſting ſacred Fire* which the *Gauers* worſhip, I ſhall now ſend you a Deſcription thereof, which you may depend upon, as there was a *Ruſſian* Army for ſome Years in the Kingdom of *Dageſtan*, where that Fire is; and I took down what I am going to relate from the Mouths and Journals of many Officers that were there, and more particularly from what was communicated to me by *Archiater Fiſcher*, who received an Account thereof from Dr. *Lerch*, Phyſician of that Army.

This perpetual Fire riſes out of the Ground in the Peninſula of *Abſcheron*, about twenty Miles from *Baku*, and 3 Miles from the *Caſpian* Shore. The Ground is very rocky, but has a ſhallow Covering of Earth over it. If a little of the Surface be ſcraped off, and Fire be applied to the Hollow, it catches immediately, and burns without Intermiſſion, and almoſt without Conſumption; nor is ever extinguiſhed, unleſs ſome cold Earth be thrown over it, by which it is eaſily put out.

There is a Spot of Ground, about two *Engliſh* Miles large, which has this very wonderful Property; and here is a *Caravanſary*, round which are many Places where the Earth continually burns; but the moſt remarkable is a Hole about 4 Feet deep, and 14 Feet in Diameter. In this *Caravanſary* live 12 *Indian Prieſts*, and other *Devotees*, who worſhip the Fire, which, according to their Traditions, has burnt

burnt many thousand Years. It is a very old vaulted Building, and in its Walls are a great many Chinks, whereto if a Candle be applied, the Fire catches instantaneously, and runs instantly wherever the Chinks communicate; but it may be easily extinguished: They have hollow Places in the House fitted to their Pots, which they boil without any other Fuel; and instead of Candles, they stick Reeds into the Ground; from the Tops whereof, upon applying Fire thereto, a white Flame immediately comes forth, and continues to burn without consuming the Reeds, until they think proper to extinguish it, by putting little Covers over them for that purpose.

They burn *Lime* of the Stones dug hereabouts, first making an Hollow in the Ground, and then heaping the Stones on one another. This done, on applying Fire to the Hollow, a Flame bursts out, and is dispersed at once with a very great Crack through the whole Heap of Stones; and after it has continued burning for three Days, the *Lime* is ready: But Stones placed in this Fire for setting their Pots on never turn to Lime; which cannot be made but by heaping them on one another. The Earth and Stone are no farther warm than where the Fire reaches: And what seems very well worth Observation, this Flame of Fire gives neither Smoke nor Smell, however great it be.

About an *English* Mile and half from this Place there are Wells of *white Naphtha*; which is exceedingly inflammable; and though the Flame of *Naptha* affords both Smoke and Smell, it is highly probable the perpetual Fire I have been describing is owing to *Naptha*, but so purified, in filtring through

through the Stone, that it becomes divested of all such Particles as produce Smoke or Smell. The Stone and Earth are grey in Colour, and saltish to the Taste; and indeed much Salt is found on this Peninsula of *Abscheron*. There is also a salt Lake, near the Side of which the *white Naptha* flows by five different Springs. This *Naptha* is made use of only in the medicinal Way. It is yellowish from the Spring, but when distilled resembles Spirit of Wine. They give it internally, for Gonorrhoea's, Disorders of the Breast, and for the Stone; and they apply it externally in gouty Cases, Contractions of the Sinews, and Cramps.

Black Naptha is produced 8 or 9 Miles from the perpetual Fire; it is thick, and being distilled grows not clear but yellow. About *Baku* there is some of it so thick, that they employ it for greasing Wheels: But the best and greatest Plenty, is at *Balachame*, where there are above 50 Springs, the greatest whereof produces every Day 500 *Batman*, each *Batman* containing ten *Russ* Pounds, which are somewhat less than *English* Weight. You hear it make a considerable Noise in rising out of the Ground, though the Spring be 20 Fathom deep.

In *Baku* they have little or no other Fewel to burn besides *Naphta*, but it must be mixed with Earth or Ashes to make it fit for Use. The Fire it makes is only good to boil with; and this Inconveniency attends it, that all their Food so boiled smells and tastes of *Naphta*. For baking and roasting they make use of *Abrotanum*, *Absynthium*, and such-like; but in general *Naphta* is their Fire.

You

You may depend on the Truth of this Account, and I hope it will be acceptable; the Hurry I am in, being Phyſician to the Army now on its March to the Aſſiſtance of the Allies, and to ſet out from this Place To-morrow, with the Commander in chief, who has been ſome time here indiſpoſed, and under my Care, prevents me from adding any more at preſent; but you ſhall be ſure to hear from me, when we are advanced into *Germany*. In the mean while, believe me to be ſincerely,

Dear Sir,

Riga, Feb. 24. 1748.

Your moſt humble Servant,

James Mounſey.

V. *An Abſtract of Mr.* Bonnet, *F. R. S. his Memoir concerning* Caterpillars; *drawn up in* French *by Mr.* Abraham Trembley, *F. R. S. here tranſlated into* Engliſh.

Read April 27. 1748.

THE Paper lately communicated by the *Preſident* from Monſieur *Bonnet* of *Geneva*, contains various Experiments he has made relating to the Reſpiration of Caterpillars.

Malpighi firſt diſcover'd, that thoſe 18 Openings or Orifices, which are placed 9 on each Side of the Caterpillar, and which are called by the Name of *Stigmata*, ſerve to give Reſpiration to this Claſs of Animals.

Monſieur

Monſieur *de Reaumur* has repeated the Experiments of *Malpighi*, and made ſeveral new ones upon this Subject. And he has been of Opinion, that theſe Apertures ſerved only for the Inſpiration of the Air, which the Caterpillar afterwards expired, through the whole Superficies of its Body. What he has wrote upon this Subject is in the firſt Tome of his Memoirs, at the 131st and the following Pages.

Mr. *Bonnet* has had Reaſon to think theſe Caterpillars do both inſpire and expire the Air by their *Stigmata*; and that they did not expire any of it through the Pores of their Body. This Paper here ſhewn gives an Account of 36 ſeveral Experiments, made chiefly with Deſign to diſcover this Fact, whether indeed theſe Inſects did both inſpire and expire the Air by their *Stigmata*, or only inſpire it. Theſe Experiments, like Mr. *de Reaumur*, conſiſt moſtly in the plunging of Caterpillars either into Water, or ſome other Liquor; ſome alſo they daubed or anointed over with fat and greaſy Subſtances, ſome quite over, and others only in ſome Places.

Mr. *Bonnet* is inclined to think, that the ſmall Bubbles of Air obſerved all over their Bodies, when they are immerged in Water, do not come from the Air included within them, and which they expired by the Pores; but that they are formed by the Air only lodged near the Surface of the Skin of the Caterpillar, as it is about the Superficies of all other Bodies: He has endeavoured to contrive it ſo, as that no Air might remain thus ſticking to the Skin of thoſe Inſects upon which he has made

these Experiments. And for this Purpose, before he plunged them in the Water, he first washed them all over with a Hair-Pencil or Brush; and these being afterwards immerged in the Water, but very few Bubbles of Air have been discovered on the Outside of their Bodies; and fewer as it appeared than Mr. *de Reaumur* had found upon those, upon which he made his Experiments; neither was this last of Opinion that all those Bubbles which he took notice of were formed by the Air rushing out through the Pores, but that some of them were also formed by the Air sticking about the exterior Part of the Skin.

When a Caterpillar is plunged in Water, one Bubble of Air is almost constantly observed upon each of the *Stigmata*. Mr. *de Reaumur* concluded, that the Air was not expired by these *Stigmata*, because he could never observe that any Bubbles of Air were ever driven out of these *Stigmata*, as one would think there must have been, if the Air was really expired by these Apertures. Mr. *Bonnet*, on the contrary, has seen some Bubbles of Air come out from these *Stigmata*, and that has contributed to make him rather think that the Air inspired was also discharged at these same Orifices. But as these Experiments are not decisive, he is unwilling absolutely to determine, but proposes the making more new Experiments.

A Caterpillar can remain several Hours under Water without perishing; it only falls into a State of Numbness; but if again taken out of the Water, it is not long before it again shews Signs of Life, and recovers. Mr. *Bonnet* has sought by some

Experiments, to know, if ſome only of theſe 18 *Stigmata* of a Caterpillar might not be ſufficient for the Purpoſes of Reſpiration: He has plunged ſome of them only partially in Water, ſometimes by the Tail, and others by the Head foremoſt; but always ſo that either 2 or more *Stigmata* might be out of the Water; and in theſe Caſes the Caterpillar has not fallen into the torpid State abovementioned, as it conſtantly did when intirely immerſed. He has lifted out of the Water ſome of the *Stigmata* of Caterpillars that had been quite immerſed, and that were ſo become torpid and motionleſs; and theſe have alſo ſoon after ſhewn Signs of Life and Motion. One of the Caterpillars, upon which Mr. *Bonnet* made Experiments, lived 8 Days, ſuſpended in the Water, and only expoſing to the Air its poſterior *Stigmata*; that is to ſay, that only the 2 laſt *Stigmata* were out of the Water.

He during this time carefully obſerved his Caterpillar; and he remarked, from time to time, when the Inſect moved itſelf, that little Streams of Bubbles came out of the anterior *Stigma* on the left Side. It appeared to him, by this and ſome other Experiments, that amongſt all the 18 *Stigmata*, the two anterior and the two poſterior ones are of a greater Uſe for the Reſpiration of the Caterpillars than any of the others. He alſo found, that, upon the choaking up theſe *Stigmata* with Butter, the Animal ſeemed to ſuffer much more ſenſibly, than when he ſo choaked up all the intermediate ones.

All theſe Experiments of Mr. *Bonnet*, and which are very particularly detailed in his Paper, were made

 with

with great Attention, Patience, and Sagacity. And it is to be wiſhed that he may continue thus diligently to apply himſelf to the Study of Natural Hiſtory.

VI. *Divers Means for preſerving from Corruption* dead Birds, *intended to be ſent to remote Countries, ſo that they may arrive there in a good Condition. Some of the ſame Means may be employed for preſerving* Quadrupeds, Reptiles, Fiſhes, *and* Inſects, *by M.* de Reaumur, *F. R. S. and Memb. Royal. Acad. Sc. Paris. tranſlated from the* French *by* Phil. Hen. Zollman, *Eſq; F.R.S.*

Read from March 10. 1747-8. *to* April 27, 1748.

PERSONS who have at Heart the Progreſs of Natural Hiſtory, and intend to facilitate the Study of it, muſt needs be deſirous to ſee the Collections of divers Sorts of Productions, which form the Objects of it, multiplied and enlarged, and therefore will be diſpoſed to contribute towards it with all their Ability. Thoſe Collections preſent together in one Place more different Sorts of Bodies of the Mineral, Vegetable, and Animal Kingdoms, there to be at Leiſure compared and examined one againſt the other, than one could hope to find ſucceſſively in the longeſt and moſt laborious Voyages and Travels. In order to render thoſe Collections

as

as complete as poſſible, there ſhould be in all the Countries of the World Men zealous for their Improvement, who ſhould take Pleaſure in tranſmitting the particular Productions of thoſe Parts which they inhabit, to ſuch Repoſitories as they know to be already conſiderable, and intended to be rendered uſeful to the Public.

That Part of Natural Hiſtory which can offer to us the largeſt Series of agreeable Objects, and actually offers a vaſt Number which are not ſought after merely for the Pleaſure of looking upon them; *viz.* that Part which treats of Birds, has remained as yet very imperfect, nor has it yet made them ſufficiently known to us, becauſe no conſiderable Collections have hitherto been made of them; and thoſe who had begun to make any ſoon became weary of going on, having had the Mortification to ſee them every Day deſtroyed by ravenous Inſects, in ſpite of all the Care that had been taken to preſerve them againſt their Teeth. M. *Reaumur* having found eaſy Methods of preparing Birds which are intended for thoſe Collections, ſo as to put them out of Danger of being ſpoiled, and to make them look as if alive, has thus found what was ſtill moſt deſired, *viz.* the means of putting them out of Harm's Way from greedy Inſects. He intends ſoon to inform the Public how to render with Succeſs this ſort of Collections durable. He has had the Luck to make one, which is already very numerous, and has Room to hope that it will be ſtill larger. The Birds, for which he is obliged to ſeveral learned Men, Lovers of Natural Hiſtory, are an Earneſt to him that he ſhall owe Thanks to them

them for more, according as they ſhall find Opportunities to procure them for him: Beſides he is ſenſible how much he may depend on their good Diſpoſition to inſtruct him, for which he is very thankful.

However deſirous one may be of ſending Birds of the Country where one lives, to another, where the like are not to be ſeen, one may be at a Loſs how to ſend them on a long Journey without their being disfigured or falling to Pieces by Corruption on the Way. I am going to explain here the different Means one may have recourſe to, for keeping them from Corruption, and to make them arrive in a good Condition.

The firſt Way.

The Method hitherto practiſed to acquaint Natural Philoſophers of very remote Countries with Birds of another Country, is to ſend them ſtuffed, that is to ſay, to take off their Skin with all the Feathers upon it, from the Body and the Thighs, leaving the Legs, the Wings, and for the better Conveniency the whole Neck with the Bill ſticking to it. Filling afterwards the Skin thus taken off with ſome ſoft Stuff, either Straw, Hay, Wool, or Flax, *&c.* or even ſtretching it over a ſolid Mould of the Shape of the Bird, you give to this Skin, as near as poſſible, the Form of the Body of the Bird, which it had when it covered its Fleſh and Bones; in which one ſometimes ſucceeds tolerably well, by Attention, and ſome ſmall Proceſſes, the Particulars of which are not intended here to be entered into.

The

The second Way.

The foregoing Way of preserving the Shape of Birds requires a Hand used to it, and even falls short of sufficiently imitating Nature, unless with Care and Time. So it is certainly most convenient only to send the Bird as it has been received. There is no great Skill required for putting one or several into a Vessel full of Spirit of Wine, or very strong Brandy. It has been usual for a long time to make use of those Liquors with Success for preserving the Flesh of dead Animals; and wherefore has this Method so seldom been used hitherto to prevent whole Birds from Corruption? Perhaps it is because their Feathers do not shew those various and bright Colours, which are natural to them, whilst they are immersed in some Liquor, and which appear no longer on the Bird's Feathers when taken out of it. Besides, the Vanes of the Feathers are then disordered, and glewed too much together. Upon these first Appearances, it was judged too hastily, that spirituous Liquors changed the Colours of the Feathers, and hinder'd the reducing of them to the Order and Pliableness they had upon the Animal, when dry and living. However repeated Experiments have made M. *Reaumur* sensible, that the Colour of the Feathers is Proof against the strongest Brandy, and even Spirit of Wine, and that after having dried the Bird that had been soaked, one may easily put its Feathers into their natural Order, and make it appear as it was when alive:

1. To preserve Birds which are to be sent far off, you are only to keep them in Brandy; the stronger it is the better it will be for producing the intended Effect: Spirit of Wine is even preferable. As for the rest, it is indifferent whether the Brandy be distilled from Wine, Corn, or Sugar.

2. Though the Birds may be put into the Liquor so as one receives them, yet some small Attention is to be had, and some Precautions to be used, before they are dipped in, which contribute towards preserving them in a more perfect State. If any of the Bird's Feathers are bloody, you must wash them from time to time with a wet Linnen, till they do not any longer leave a Mark upon that Linen, or in the Water in which they are soaked. Above all it is of Consequence to hinder the Feathers from taking a wrong Bent, or rumpling. It is easy to put them into the Shape they are to be, by smoothing them with a Finger from the Head towards the Tail in squeezing them together. This helps the Feathers to take the Position which is most natural to them, and in this Position they are kept by wrapping the Bird up in a Rag, tying about the Neck and the Body several times a strong Packthread: The Feathers on the Neck are chiefly those which must be kept from turning aside or backwards.

3. The Precaution of taking out of the Body the Intestines and other Parts it contains, is not absolutely necessary; it is better however to do it: If afterwards one supplies their Place, by filling the Cavity of the Belly with all the Quantity it can contain of Wool, Hemp, Cotton, or other soft Matter; if you fill the Neck, though without dis-

tending

tending it, with the same soft Matter, you will more surely preserve the Shape and Dimensions of the Bird. It becomes less big in the spirituous Liquor; not just because the Flesh shrinks or dries up, but because the Parts which form the Cavities endeavour to contract them, and in effect will do so, if the Cavities do not contain a Matter which resists.

4. After these plain and easy Preparations, you are only to put the Birds into the Vessel containing the Liquor which is to preserve them. This Vessel may be a Jar of Glass, if it is only intended for receiving small Birds; one may contain a great Number of them, which you may put in at different Times, accordingly as you get them, till it is quite full. Wooden Barrels however are preferable to Jars, as they are not liable to break in long Journeys; there are to be had very small ones for smaller Birds, and some large enough for others of the tallest Size. The Barrel is to have a Hole large enough for passing the Birds through: This Hole can be no other than the Bung widened, it will even be better placed in one of the Heads. It is unnecessary to advertise that it ought to be kept shut up with a Stopple of a proportionable Diameter, except during the short Time when it is opened for putting the Bird in.

5. The Birds may be sent in the very Jars or Barrels where you have put them: But if they are to be on the Road for several Months, or for Years, you will renew the Liquor before you send them: That which has been poured on at first, may have

been weakened by Evaporation, and by the aqueous Juices extracted from the Flesh.

6. If those Birds are not to arrive by Sea to their Journey's End, if they are to be cartied by Land for Part of their Way, one must contrive it so, that they may not be liable to be tossed by much Jumbling; and they will be less so, if the Vessel is so much fuller of them; they will close the more together. In case they should float too much in the Liquor, you need not scruple to press them with Hay or some other Stuff, which you thrust into the Vessel.

7. It is still more easy to hinder the Birds from being tossed, and they will even be the better preserved, if before you send them you take them out of the Liquor, in which they have lain a sufficient Time; it has made them fit to dry without any Danger of Corruption. Small Birds, such as of the Bigness of Sparrows, and even of Black-birds, after having been cover'd 8 or 10 Days with strong Brandy, may be taken out without any Fear of their being corrupted. Large Birds, and especially such as are very fleshy, are to be kept longer in the Liquor; but there are none or few, for which it may not be enough to have lain in it a Month or five to six Weeks. According as you take them out, you must range them one next to the other, and upon one another in a Box, filling up the Intervals with a Matter easiest to be had, as Chaff of Oats or Barley; that is to say, those small Shells in which the Grain was wrapped up whilst it stuck to the Ear. This Chaff is the best Stuff for this Use: You may also use small Hay, Moss, Hemp, Cotton, &c. Far from its being necessary to leave the

the Birds to dry before ranging them in the Box, the beſt is to put them in quite dripping with the Liquor. Having filled the Box well, there remains only to ſhut it up.

8. Any Box, of what Form ſoever, may be fit for Birds which are to be on the Journey only for ſome Weeks or a few Months: Such as are to travel Years, require more Precaution; though they are not ſubject to Corruption, yet they may be torn to Pieces before their Arrival, if Inſects greedy of them can come at them, and multiply in their new Habitation One may by Care ſo well cloſe up thoſe Boxes, as to render it impoſſible for thoſe dreadful Inſects to get to the Inſide; Paper glued over all the Joints will prevent it. But Barrels are preferable to Boxes, for ſuch Birds as are to remain ſhut up for a Year or longer; the ſmalleſt Inſects will not find a Paſſage for creeping into a Barrel, which will not permit the ſmalleſt Drop of Liquor to get out. Birds being put wet into the Barrel, keep from drying up too much, and keep one another the cloſer. As good Luck will have it, carnivorous Inſects are none of thoſe that will pierce Wood. So by uſing Spirit of Wine or ſtrong Brandy, as we juſt now ſaid, one will ſucceed in having thoſe Birds arrive in a good Condition at the remoteſt Places. There is ſtill another Way for it, which may appear more convenient, eſpecially for Birds of a large Size.

The third Way

Is to preſerve Birds by a ſort of Embalming, and even by actual Embalming, in Countries

where the Spices are cheap. First, you begin with emptying the Body of the Bird, and then fill it with those Powders I am going to specify to you; you also fill its Neck with the same Powder, thrusting it in through the Bill. If the Bird is extremely fleshy, you may make an Incision in the Flesh of the thick Part of each Leg, and one in the Flesh of each Wing; that is to say, two on the Breast, and one nearer the first and large Bone of each Wing, into which you put the Powder; having afterwards brought the Flesh together again, and put the Feathers in Order, those Incisions will be hid so as not at all to disfigure the Bird. But there are very few on which it was necessary to make such Incisions; one may make some even inwardly, which will serve as well; having thrust your Fingers into the Belly, you may tear the Integuments over-against the thick Part of the Leg, and in other Places, and make Cavities to be afterwards filled up with the Powder.

2. There are many Powders proper to produce the principal Effect intended here, which is to promote the Bird's drying before it be so far corrupted as to occasion the falling off of the Feathers. All sorts of Spices may be used for it with Success; if there are any in the Country which are very cheap, you may use them. You may even make use of a Powder composed of as many Sorts of Spices as you will, the Result of which will be at least, that the Bird, after being dried, will smell the sweeter, and become as it were a Piece of Perfume. But instead of using resinous Gums, as Aloe, Myrrh, Frankincense, and other Productions of Plants, as Cinnamon, Cloves, Pepper, Ginger, *&c.* which are dear Materials,

terials, you may content yourself with a Salt which is cheap in most Countries; it is sufficient to fill the Cavity of the Body and of the Neck with Alum reduced to Powder. A Material still easier to be had in all Places, and very cheap, and which works with great Effect, is Lime. If it can be had quite unslack'd, you will take it preferably; however, without scrupling to take such as is old, and which has been somewhat slackened by the Humidity of the Air.

After the Body and the Neck of the Bird have been filled up, either with pulverized Lime, Alum, or any other Powder, you put it into the Box or the Barrel, in which it is to be transported. You will take care, in placing it, to give a natural Position to the Neck, neither to give to the Legs any other Inflexion than they had when the Bird stood upon them alive. At the Bottom of the Box or the Barrel there is to be a Layer of the Thickness of an Inch, or thereabouts (if there be more there will be no Harm) of the same Powder with which the Cavity of the Body is filled, or of any of those which are proper for it. You bury the Bird in this Powder, and put enough of it about it and upon it, so as to cover it with a Layer of the Thickness of an Inch or more. The outward Powder will make it dry the sooner, and keep off voracious Insects, which will not care to attempt to pierce through it in order to come to the Flesh they are fond of. During the first Days, and even during the first Weeks, the Birds may cast a bad Smell, which you need not be uneasy at, for it will lessen in proportion to the Bird's drying; and it will dry so

ſo that none of the Feathers will come off; and when it is once dried, they ſtick faſt to it for ever. This Way of preſerving Birds, which is very ſimple, has procured to M. *Reaumur* ſome from very remote Countries, which arrived as wiſhed for.

The fourth Way.

This is one, by which Birds are more ſpeedily dried, than by that which is explained before; it is to dry them by the Heat of an Oven. You make uſe of that Heat which remains in it after the Bread is taken out of it; ſometimes it is then too great, but there is a plain Way to be ſure that the Degree of Heat is not too great, which is, to put Feathers into the Oven, and to take them out 5 or 6 Minutes after; if you find that they are not ſinged, nor turned red, you ought not to be under any Apprehenſion for the Feathers of the Bird, which is to be put into the Oven. Small ones need remain in it only one or two Hours to be ſufficiently dried; thoſe of a middling Size require a longer Time; and thoſe which are big, and very fleſhy, ought to be put in at ſeveral times. When they are grown cold, you may know whether they are dried enough, by preſſing with the Finger the Fleſh of the Legs and of the Breaſt; if it does not yield, or yields but little under the Finger, the Bird does not any more want to be put into the Oven. The Inconveniency attending its being kept there longer than is neceſſary, is, that ſome Parts of it, as for inſtance, the Neck and the Rump, are thereby render'd too brittle. You will prevent the Bird's Bulk ſenſibly di-

minishing

minishing in the Oven, if, before you put it in, you fill the Cavities of its Body and the Neck with some soft Stuff, like any of those which we mention'd to be us'd for filling the Cavities of such Birds as are intended to be preserved by the means of Spirit of Wine, *viz.* Hemp, Flax, Cotton, *&c.* What is the most difficult in the Way of drying Birds in the Oven, is not hitting the proper Degree of Heat, and to know the Time how long they are to be kept in it: Here will be the Difficulty, how, as this Way of drying requires the Bird may be kept in a natural Attitude, before it is put into the Oven: If dried, it will be fixed for ever in that which it once received. There are several Ways, plain in themselves, for putting and keeping the Bird in its natural Attitude, which however would be too long to be explain'd as to the Particulars; the little we shall say of them, will be sufficient to industrious Persons for their Use. The Bird may be kept in Order by the means of a Frame, made like a Farrier's Travise; it is composed of a small Board, which forms the Basis of it, the Length of which need not be greater than that of the Bird: On each Side of this Board rises an upright Post of Wood; these four Posts are secured by Traverses fixed to them by small Nails: The Use of those Posts and Traverses is to keep fixed the small Ribbons and Threads, which keep the Body, the Wings, and the Neck of the Bird in the Position it has been brought to. A Thread run through the Head of the Bird, with the Help of a Needle, enables you to place it as high or low as you please. There are various Ways of fixing the Feet on the Board, with the Claws extended;

extended; it may be done with ſmall Points of Nails. With a Wire only, and a ſmall Board, all may be done as well as with a Frame: This Wire is run through all the Length of the Body and of the Neck of the Bird, by inſinuating it through the *Anus*; but before doing ſo, you make a ſort of a ſtrong Knot to it, by twiſting it; this Knot is to touch the *Anus*; it afterwards hinders the Bird from ſliding: Cloſe by the Knot you bend down perpendicularly that Part of the Wire which is without the Body, and which is to be at leaſt of a Length equal to the Height which the Legs are to have; you make afterwards its End pointed by filing, if you have not already done it, and you run it into the Board. That Part of the Wire which then is out of the Body, ſerves for a Supporter, which keeps the Bird raiſed, becauſe it is continued to the reſt of the Wire which runs through the Body and the Neck: The Wire which runs through the latter keeps it in its bending Way, and the Direction that has been given to it.

Dried Birds ought to be ſent in Boxes or Barrels ſufficiently cloſed up, that Inſects may not ſlip in during the Journey; and you will take care to fill up all the empty Spaces left in the Barrel with ſome of thoſe ſoft Stuffs, which we have already pointed out for ſuch Uſes. Many Weeks, nay even Months, may paſs between the Time, when you have dried the firſt Birds you intend to make a Collection of for a Journey, and that Time when they are to ſet out: This Interval is dangerous. There are certain Worms, and certain Beetles, which are more greedy after thoſe dried in the Oven, than after thoſe dried

any

any other Way; if they meet with free Accefs, they fometimes feize the firft Moments to fettle under the Feathers, or in the Bodies, where they multiply.

You will put your Birds out of the Reach of the formidable Teeth of thofe Infects, if after they have been taken out of the Oven, you bury them in Sand contained in a large Box or a Barrel. You muft take care in covering them with Sand, that they may not contract bad Attitudes, and that their Feathers be not ruffled. Slack'd Lime reduced to Powder, Chalk, and all earthy Powders, fine and dried, may be fuccefsfully employ'd for the fame Ufe. You will prefs with your Hand the Surface of the Powder, to render the uppermoft Lay compact, which is very neceffary. Laftly, if from the falling of the Feathers it appears that the Infects have defeated the Precautions taken againft them, there is ftill a Remedy left; you may ftop the Progrefs of the Evil by putting the Bird again into the Oven, not hot enough to finge the Feathers, but hot enough to kill the Infects in lefs than half an Hour.

Remarks that are common to the four Ways of preparing Birds.

1. It will not be amifs to fend two or three Birds of each fort; and, as near as you can, let there be one Male and one Female.

2. One cannot help being curious to know the Name which each Bird bears in the Country where it was taken: You write it with common Ink upon a Slip of

 Parchment,

Parchment, which you tie with a Thread to one of its Feet; the Writing will be preſerved, even when the Bird is in Brandy.

3. If you know of a Bird any thing beſides its Name, you may make a ſmall Note, ſhewing in what Places it lives; what it feeds upon; whether or no it ſtays all the Year in the ſame Country; how and where it makes its Neſt; how many Eggs it lays; the Wiles and Cunnings particular to it; whether it is good to eat; in ſhort, whatever is known of its Hiſtory.

4. A Collection of Neſts is a proper Repoſitory to be joined to that of Birds; it ſhews ſuch Works as hardly could be imitated by Men, admirable for their Form, their Workmanſhip, and the Materials employed in them: M. *Reaumur* has already made ſuch a Repoſitory. If one can have Neſts not too bulky for eaſy Tranſportation, you may be ſure to ſee them with Pleaſure joined to the Birds that have built them.

5. The Colours and Figures of the Eggs make alſo Part of the Hiſtory of Birds; Collections made of them will give Satisfaction to curious Minds: Thoſe which are to be ſent would be in Danger of being broken on the Way, by the very Subſtance they contain, if it comes to ferment. Before you ſend them therefore, you muſt empty them: To this end you make a ſmall Hole on each End, and ſhake them; and if this Shaking will not be enough, you blow into one of the Holes to force out through the other what liquid Matter remains in the Egg.

State

Quadrupeds.

Quadrupeds that are not of too large a Size, and particular to certain Countries, may be put into a State fit to be ſent to the moſt remote Parts, by one of the four Ways uſed to preſerve Birds: You may make durable Collections of them like thoſe of the latter. M. *Reaumur* has begun one, which makes Perſons who ſee it wiſh that there might be more complete ones of the ſame kind.

Fiſhes and *Reptiles.*

Fiſhes and Reptiles, which, as well as Quadrupeds, are engaging Objects for Naturaliſts, are eaſier to be ſent; it is ſufficient to put them into Barrels full of ſtrong Brandy. They may alſo be dried, either by Materials with which you may fill the Cavities of their Bodies, or by a gentle and well-manag'd Heat.

Inſects.

Inſects, which offer to us ſo many admirable Varieties, deſerve the Care of gathering them into Collections, which cannot but be precious to thoſe who have made thoſe little Animals their Study. All thoſe which are ſoft, as for inſtance Worms and Caterpillars, may be preſerved in Brandy. Their tender Colours will run leſs Hazard of being alter'd, if you put into the Brandy ſuch a Quantity of Sugar as it is able to diſſolve. Beetles may alſo be put into the ſame Liquor; but Butterflies and Flies would be ſpoiled in it: After having killed them, you muſt range them in Lays in Boxes, and ſeparate thoſe Lays with Beds of Cotton. Though one ought to collect in each Country, preferably, thoſe which ſtrike

moſt, either by the Variety and Luſtre of their fine Colours, or by their Size, or by their ſingular and odd Form, or by the Uſe one knows to make of of them; yet you ought not to neglect to collect and ſend ſuch as do not offer ſo remarkable Singularities, ſuch as even are moſt common. There are among the latter ſome, which have wherewithal to ſatisfy an Obſerver, who looks upon them with other Eyes than thoſe wherewith they had been regarded before, and with other Views.

VII. *A beautiful* Nautilites, *ſhewn to the* Royal Society *by the Rev.* Charles Lyttleton *LL.D. F. R. S. and Archdeacon of* Exeter.

Shewn May 5. 1748.

THIS curious Foſſil ſeems to be compoſed of a ſtony Matter like Marble, which has penetrated the Cells of the *Nautilus* while in its natural State. The Diaphragms or Partitions remain ſtill diſtinct and viſible. The different Colour of the ſtony Matter in ſome Cells of a dark-brown or Hair-Colour, in others of a light-brown or Aſh-Colour, with the natural Poliſh of the Outſide, gives it a beautiful Appearance; as it is repreſented in the annexed Print (*See* TAB. *Fig.* 2.) where it is drawn of its natural Size in three different Views.

A ſhews the Side View of it.
B the fore Part.
C the back Part.

C. M.

It

It was found in *Pool's Hole* in *Derbyshire*. Its Sutures or Diaphragms resemble those of some of the larger *Cornua Ammonis*; but its Shape bespeaks it to be a Species of *Nautilus*; and it is thought to be a Non-descript, both in its natural and fossil State.

VIII. *The Substance of a Letter from Mr.* William Arderon *F. R. S. to Mr.* Henry Baker *F. R. S.*

Read May 12. 1748.

OF all the several Kinds of Fish which for some Years past I have been keeping in glass Jars (in hopes of becoming acquainted with the Nature and Properties of these Animals, by having them daily under my Inspection) none seems more impatient of Imprisonment, if I may so call it, than the Roach; nor, if they are well look'd after, and supplied often enough with fresh Water, have I observed any, except the Roach, to become distempered. But most commonly, after this Fish has been a little while confined, the finny Part of its Tail begins to drop off Piece by Piece; and when the finny Part is all gone, a sort of Mortification seizes upon the Tail itself, and gradually creeps along until it reaches the Intestines, at which time the Fish immediately dies.

The last Roach I had under this Disorder was about the Beginning of *January*; when in the Space of a Month, it had lost the greatest Part of the Fin, which induced me to clip off the rest, hoping thereby

thereby to ſtop the Progreſs of the Mortification. But this was of no manner of Service that I could perceive: The Diſtemper ſtill gained Ground; and as it increaſed, a fine fibrillous Subſtance grew out from it, and appeared like what the Picture ſhews at *Fig*. 3. in TAB.

Theſe Fibrils, when examined by the Microſcope, ſhew themſelves to be a Number of minute Tubes, filled with a browniſh Liquor; and this Liquor, upon preſſing them, becomes immediately diſcharged.

A ſmall Piece of this Fiſh, with the Fibrils growing out of it, as ſeen by the fourth Magnifier of Mr. *Cuff*'s double Microſcope, is ſhewn at *Fig*. 4.

When firſt I perceived this fibrous Subſtance inveloping the Fiſh's Tail, I ſuppoſed it to be nothing but a Mouldineſs, of that kind which frequently is ſeen upon decayed Fleſh and Fiſh; but, upon Trial, I found it to be of a much ſtronger Texture and Conſiſtence than ſuch Mouldineſs is ever known to have; for, notwithſtanding I have ſeveral times let a full Stream of Water run upon it from a Cock, I could never waſh it off.

This Fiſh lived with me till the latter End of *March*, and then died; having for many Days before its Death lain at the Bottom of the Jar, without being able to riſe.

As the Mortification advanced, and came nearer to its Inteſtines, the Quickneſs of its taking Water in at the Mouth increaſed, till at laſt it took it in three times faſter than a lively ſtrong Fiſh did.

On my cutting off Part of the Fiſh's Tail, in hopes of ſtopping the Mortification, the Equilibrium of the Body was ſo far loſt, that it hung in the Water moſt commonly with the Head downwards,

4 and

and could never afterwards continue in any other Posture, without great Strugglings, or sinking down to the Bottom of the Vessel. Which may serve to shew how nicely and wonderfully the Bodies of Fishes are balanced, for the keeping them in an horizontal Position; since in this Case the losing a few Grains of the Tail could so sensibly destroy the Equilibrium, as to render the rest of its Fins almost useless.

I dare not however assert it will happen thus to all sorts of Fish on cutting off the Tail; nor does it to the Roach immediately: For as it is a Posture very unnatural and troublesome to Fishes, they exert all their Strength to prevent their Heads from sinking downwards; until, being wearied out, they at last are forced to submit. I remain,

Dear Sir,

Norwich, April 14. 1748.

Your most obedient Servant,

W. Arderon.

IX. *A Letter from Mr.* Robert Roche *to the* President, *of a* Fustian Frock *being set on Fire by* Electricity.

Honoured Sir, *London, May* 17, 1749.

Read May 19. 1747.

IF your Goodness will excuse the Liberty a Stranger has taken in giving you this Trouble, hoping the following Account will

will atone for his Boldneſs, I ſhall think myſelf greatly favoured.

I have a Son about 16 Years old, that has been for 6 or 7 Years paſt troubled with ſudden Fits that intirely take away his Senſes. I got him all the Helps I could, but to no purpoſe; at laſt I ſent him to St. *Bartholomew's* Hoſpital, as an Out-Patient; and there he was turn'd out as incurable. So finding his Caſe deſperate, I conſidered the Power of Electricity, and made a large Machine for Electrifying; and afterwards ſhocking him commonly twice a Day, he has received ſome Benefit: And laſt *Sunday*, being *May* 15, he being on the Pedeſtal, and very high electrified, and having on a coarſe Fuſtian working Frock, the condenſing Phial being on the Conductor, and I, touching him to procure Snaps as uſual, touched his right Shoulder Blade; and, to my great Surprize, the furzy Flax of the Frock caught Fire, with a great Blaze, and burnt the whole Breadth and Length of the Shoulder, the Flame riſing 6 Inches above the Collar, and I believe would have ſet the Frock on Fire, had I not put it out with my Hands. There was no Fire in the Room that Day: This was about Noon; neither was there any thing that could have any inflammable Vapour there.

My Surprize was the greater, becauſe all I read on that Subject ſays nothing will burn but what ſends forth ſuch Vapours.

At 9 the ſame Evening I made him put on the ſame Frock, and touch'd the left Arm, where the Flax had not been burnt before; and it had the ſame Effect as above.

Sir,

Sir, if any further Account of the above will be acceptable to you, or the *Royal Society*, if you pleaſe to command I will wait on you. There are Alterations in my Machine I think for the better; and ſome new Experiments too long to write, fearing it would be too troubleſome; from

Your humble and obedient Servant,

R. Roche.

X. *A Letter from* John Huxham *M. D. F. R. S. to* C. Mortimer *Secr. R. S. concerning a* Child *born with an* extraordinary Tumour *near the* Anus, *containing ſome Rudiments of an Embryo in it.*

Dear Sir,

Read May 19. 1748.

THE following Caſe was lately communicated to me by Mr. *Alexander Wills*, an experienced and ingenious Surgeon and Man-Midwife, of *Kingsbridge*. It ſeems to have ſomething particular and remarkable. If you think ſo (on Peruſal) you may be ſo good as to lay it before the *Royal Society*. I am,

SIR,

Plimouth, Dec. 12. 1746.

Your much obliged, and moſt obedient humble Servant,

J. Huxham.

" *John Perrine*'s Wife, of *Charleton* Pariſh, in " this County, a brisk active young Woman (he " very infirm and conſumptive) was deliver'd of a " Daughter at full Time, *July* 11, 1746. The " Child was perfect, as to all its Limbs, Head, " Body, *&c.* but from the Region over the *Os* " *ſacrum*, *Glutæi* Muſcles, and between the Thighs, " quite home to the *Pudendum*, was growing a " very large Subſtance, which the Midwife and " others call'd a Wen, in Shape very like the Ven- " tricle of a Sheep, and ſeem'd, as to its Colour " and outward Appearance, a Continuation of the " ſame Skin with the reſt of the Body, but very " full of Blood-veſſels. It hung down behind be- " low the Heels, and was bigger than the whole " Body of the Child itſelf. It felt very ſoft, and " ſeem'd to have Matter fluctuate in it; but in the " middle of the Whole was evidently felt a hard " Subſtance.

" The *Pudendum* as well as *Anus* were in all re- " ſpects natural, and both Urine and Stool were re- " gularly diſcharg'd; but the *Anus* was placed much " more forward, and immediately under the *Pu-* " *dendum*; ſo that the *Fæces* were diſcharged in " the ſame Direction with that of the Urine.

" I made a Puncture in the depending Part of " the Tumour, and drew off near two Quarts of a " paliſh red Water, without any Smell. The Ori- " fice being left open, there was a continual Iſſue " of the ſame kind of Water for ſeveral Days; but " by degrees it became more and more glutinous, " and at length whitiſh like *Pus*, and very fetid. As " the

" the Discharge was great, the Child grew weaker " and weaker, and at the End of 15 Days died.

" The next Day I open'd the Tumour, and found, " near the *Os Coccygis*, an Abscess within a Cystis, " in which were four Ounces at least of white " *Pus* prodigiously stinking; and, on further Exa- " mination, found several cartilaginous Joints, as it " were, somewhat resembling the Tail of a Sheep, " continued from the Point of the *Os Coccygis*. " These were about two Inches long, and invelop'd " with a kind of fleshy Substance cover'd with a " sort of Fat: These, when cut thro', appear'd ex- " actly like the inner Part of Lamb-Stones. From " those depended a Substance like the Head and Neck " of an Embryo, as big as a large Egg, which, on " opening, contained somewhat resembling Brain, " and a kind of a *Cerebellum* in the back Part: " It had a Mouth and Tongue on one Side of the " Face (if it might be so call'd) but no Appear- " ance of Eyes or Nose; however there was an " Ear pretty evident.

" In the large Tumour there hung a kind of " loose Membrane, which perhaps might be Part " of a Secundine.

XI. *Of the Fluents of* Multinomials, *and Series affected by* radical Signs, *which do not begin to converge till after the second Term; in a Letter from* T. Simpson *F. R. S. to* W. Jones *Esq; V. P. R. S.*

Presented May 26. 1748.

ALTHO' the Application of infinite Series, and the Quadrature of the conic Sections, to the inverse Method of Fluxions has exercised the Pens of the most able Mathematicians, and produced many curious and useful Discoveries, yet nothing has been hitherto given, that I know of, whereby the Fluents of radical Multinomials and Series, which do not begin to converge till after the second Term, can be determined, so as to be of Use in the Solution of Problems: The common Method, by expanding the given Expression, being, you know, altogether impracticable in this Case.

The Consideration of which induced me to draw up the following Paper; which I humbly beg Leave to lay before you, who are so good a Judge of the various Improvements which this Subject has from time to time received.

What most encourages me to hope this little Essay will meet with your Approbation, is, that it is not merely an abstracted useless Speculation, but may be apply'd to good purpose in many difficult and important Enquiries into Nature; whereof I have put down one or two Instances, and shall further take the Liberty to observe here, that most of the

lunar

lunar Equations, given by Sir *Isaac Newton*, are only ſuch Approximations as may be exhibited by the firſt Term of a Series derived by the Method here delivered.

Propoſition.

The Fluent of $\overline{a+cz^n}|^m \times z^{pn-1}\dot{z}$ *being given (either in algebraic Terms, or from the Quadrature of the Conic Sections, &c.) it is propoſed, by means thereof, to approximate the Fluent of* $\overline{a+cx^n+dx^{2n}+ex^{3n}+fx^{4n}\ \&c.}|^m \times x^{pn-1}\dot{x}$; *ſuppoſing the Series not to converge till after the ſecond Term.*

Make $cz^n = cx^n + dx^{2n} + ex^{3n}$ *&c.* and let Q be the given Fluent of $\overline{a+cz^n}|^m \times z^{pn-1}\dot{z}$, anſwering to any propoſed Value of x: Moreover let $y = x^{pn}$, or $y^{\frac{1}{p}} = x^n$, and let this Value of x^n be ſubſtituted in the firſt Equation, and it will become $cz^n = cy^{\frac{1}{p}} + dy^{\frac{2}{p}} + ey^{\frac{3}{p}}$ *&c.* whereof the Root y being extracted, we ſhall (by making $R = -\frac{pd}{c}$, $S = \frac{p.p+3}{2} \times \frac{d^2}{c^2} - \frac{pe}{c}$, $T = \frac{-p.p+4.p+5}{6} \times \frac{d^3}{c^3} + \frac{p.p+4}{1} \times \frac{de}{c^2} - \frac{pf}{c}$ *&c.*) have $y\ (x^{pn}) = z^{pn} + Rz^{pn+n} + Sz^{pn+2n}$ *&c.* whence we alſo obtain $x^{pn-1}\dot{x} = z^{pn-1}\dot{z} + \frac{p+1}{p} \times Rz^{pn+n-1}\dot{z} + \frac{p+2}{p} \times Sz^{pn+2n-1}\dot{z}$ *&c.*

Let this Value, with that of $cx^n + dx^{2n} + ex^{3n}$ *&c.* (above given) be now ſubſtituted in the pro-

poſed

poſed Fluxion, and it will become $\overline{a+cz^n}|^m \times z^{pn-1}\dot{z} + \frac{p+1}{p}\times Rz^{pn+n-1}\dot{z} + \frac{p+2}{p}\times Sz^{pn+2n-1}\dot{z}$ &c.

Moreover, let v denote the Place, or Diſtance, of any Term, of this Expreſſion, from the firſt (excluſive) then the Term itſelf (drawn into the common Multiplicator) will be denoted by $\overline{a+cz^n}|^m \times \frac{p+v}{p} \times Az^{pn+vn-1}\dot{z}$; and the Fluent thereof will be truly expreſſed by $\frac{p+1}{p+m+1} \times \frac{p+2}{p+m+2} \times \frac{p+3}{p+m+3} \times \ldots \frac{p+v}{p+m+v} \times \overline{\frac{a}{-c}}|^v \times AQ + \frac{\overline{p+v}.A}{p} \times \frac{\overline{a+cz^n}|^{m+1} \times z^{pn-n}}{\overline{p+m+v}\times nc}$ into $z^{vn} - \frac{p+v-1}{p+m+v-1} \times \frac{az^{vn-n}}{c} + \frac{\overline{p+v-1}.\overline{p+v-2}}{\overline{p+v+m-1}.\overline{p+v+m-2}} \times \frac{a^2z^{vn-2n}}{c^2}$ &c. continued to as many Terms as there are Units in v. Wherein let v be expounded by 1, 2, 3 &c. ſucceſſively, and R, S, T, &c. by A reſpectively: By which means the Fluent of the whole Expreſſion will be obtained.

Corol. 1.

Becauſe the Fluent of the general Term, when the Multiplicator $\overline{a+cz}|^{m+1}$ becomes $=0$, is barely $=\frac{p+1}{p+m+1} \times \frac{p+2}{p+m+2} \times \frac{p+3}{p+m+3} \times \ldots \frac{p+v}{p+m+v} \times \overline{\frac{a}{-c}}|^v \times AQ$ the Fluent of the whole Expreſſion will, therefore, in this Caſe be truly defined by

4 Q

$\mathcal{Q} \times \overline{1 - \frac{\overline{p+1}.\,Ra}{\overline{p+m+1}.\,c} + \frac{\overline{p+1}.\,\overline{p+2}.\,S_2a}{\overline{p+m+1}\,\overline{p+m+2}.\,c^2} - \frac{\overline{p+1}.\,\overline{p+2}\,\overline{p+3}.\,Ta^3}{\overline{+m+1}.\,\overline{p+m+2}.\,\overline{p+m+3}.\,c^3}}$ &c. Where $\mathcal{Q}$ denotes the Fluent of $\overline{a+cz^n}|^m \times z^{pn-1}\dot{z}$, when $z^n = \frac{a}{-c}$.

Corol. 2.

But, if $m+1$ and p be, each of them, the Half of an odd affirmative Number, and $\mathcal{P}$ be taken to denote the Periphery of a Circle whose Diameter is Unity, and $-c$ be put $=b$, then the Valne of $\mathcal{Q}$ (or the Fluent of $\overline{a-bz^n}|^m \times z^{pn-1}\dot{z}$, when $z^n = \frac{a}{b}$) will be $= \frac{a^{p+m}\,\mathcal{P}}{nb^p} \times$

$\frac{1.\,3.\,5.\,7\ \&c.\ (\text{to } p-\frac{1}{2} \text{ Factors}) \times 1.\,3.\,5.\,7\ \&c.\ \text{to } (m+\frac{1}{2} \text{ Factors})}{2.\,4.\,6.\,8.\,10.\,12\ \&c.\ (\text{to } p+m \text{ Factors})}$

Therefore the *Whole*, required, Fluent, of $\overline{a-bx^n+dx^{2n}+ex^{3n}\&c.}|^m \times x^{pn-1}\dot{x}$ is, in this Case, equal to the Product of that Expression into the following Series, $1 + \frac{\overline{p+1}.\,Ra}{\overline{p+m+1}.\,b} + \frac{\overline{p+1}.\,\overline{p+2}.\,Sa^2}{\overline{p+m+1}.\,\overline{p+m+2}.\,b^2}$ &c. Wherein R is to be taken $= \frac{pd}{b}$, $S = \frac{p.\,\overline{p+3}}{2} \times \frac{d^2}{b^2} + \frac{pe}{b}$, $T = \frac{p.\,\overline{p+4}.\,\overline{p+5}}{6} \times \frac{d^3}{b^3} + \frac{p.\,\overline{p+4}}{1} \times \frac{de}{b^2} + \frac{pf}{b}$, &c. according to what is above specified.

The Use of what has been deliver'd above will, in some measure, appear from the Solution of the two

two following Problems, which I ſhall ſubjoin as Examples thereof. The firſt is;

To find the Time of Oſcillation in the Arch of a Cycloid, in a Medium reſiſting according to the duplicate Ratio of the Velocity.

Let A denote the whole Arch of the Semi-Cycloid, or the Length of the Pendulum, a the the Arch deſcribed in the whole Deſcent, and x any variable Part thereof deſcribed from the Beginning of the Deſcent; and let the Denſity of the Medium be, every-where, as $\frac{1}{b}$: Then the Fluxion of the Time will be found $=$

$$\overline{a-\overline{1+\frac{2a}{b}}\times\overline{\frac{x}{2}-\frac{2x^2}{2.3b}+\frac{4x^3}{2.3.4b^2}-\frac{8x^4}{2.3.4.5b^3}\text{ \&c.}}}\Big|^{-\frac{1}{2}}\times$$

$\overline{2A}\big|^{\frac{1}{2}}\times x^{\frac{1}{2}}\ \dot{x}$ *: which being compared with $\overline{a-bx^n+dx^{2n}+ex^{3n}\text{ \&c.}}\big|^m\times x^{pn-1}\dot{x}$ (*vide Corol.* 2.) we ſhall, in this Caſe, have $n=1$, $m=-\frac{1}{2}$, $p=\frac{1}{2}$, $a=a$, $b=\overline{1\times\frac{2a}{b}}\times\frac{1}{2}$, $\frac{d}{b}=\frac{2}{3b}$, $\frac{e}{b}=-\frac{1}{3b^2}$, $\frac{f}{b}=\frac{2}{15b^3}$ &c. Whence $R=\frac{1}{3b}$, $S=\frac{2}{9b^2}$, $T=\frac{8}{45b^3}$ &c. Alſo $\frac{a^{p+m}P}{nb^p}\times\frac{1.3.5.7\,(p-\frac{1}{2})\times 1.3.5.7\,(m+\frac{1}{2})}{2.4.6.8.10.\,(p+m)}=\frac{P}{b^{\frac{1}{2}}}$, and $1+\frac{\overline{p+1}.Ra}{\overline{p+m+1}.b}+\frac{\overline{p+1}.\overline{p+2}.Sa^2}{\overline{p+m+1}.\overline{p+m+2}.b^2}$ &c. $=1+\frac{a}{2bb}+\frac{5a^2}{12b^2b^2}$

* The Inveſtigation of this, and the Fluxion in the following Example, are both given in my Eſſays.

$+ \frac{5a^2}{12b^2b^2} + \frac{7a^3}{18b^3b^3}$ *&c.* Whence we have $\overline{2A}|^{\frac{1}{2}} \times \frac{P}{b^{\frac{1}{2}}} \times \overline{1 + \frac{a}{2bb} + \frac{5a^2}{11b^2b^2}}$, *&c.* for the Time of one Vibration of the Pendulum; which, by ſubſtituting $\overline{1 + \frac{2a}{b}} \times \frac{1}{2}$ for its Equal b, *&c.* becomes $PA^{\frac{1}{2}} \times \overline{1 * + \frac{a^2}{6b^2} - \frac{2a^3}{9b^3}}$ *&c.* From which it appears, that the Effect of the Reſiſtance on the Time of Vibration, in ſmall Arches, is nearly in the duplicate Ratio of thoſe Arches.

Sir *Iſaac Newton* (from whom it is impoſſible to diſagree without being under ſome Apprehenſions of a Miſtake) has, indeed, given a very different Solution to this Problem (in *Princip. Prop.* 27. *B.* 2.). But as the Concluſion here brought out exactly agrees with what I have elſewhere given, by a different Method, I have great Reaſon to believe I have no where fallen into an Error.

The ſecond Example I ſhall give as an Illuſtration of the foregoing Method is,

To determine the Apſide Angle (or the Angle of the two Apſes at the Center) in an Orbit deſcribed by means of a centripetal Force, which varies according to any Power of the Diſtance.

In order to which, let the Velocity of the Body at the higher Apſe be to that whereby it might deſcribe a Circle at the ſame Diſtance from the Center, in the given Ratio of p to Unity; alſo let

that Distance be denoted by Unity; and, supposing z to denote any other Distance, let the centripetal Force be universally expressed by z^n. Then the Fluxion of the Angle at the Center will be expressed by

$$\frac{-p\dot{z}}{z\sqrt{p^2+\frac{2}{n+1}\times z^2-p^2-\frac{2z^{n+3}}{n+1}}}.$$

Put $a=1-p^2$, $v=\frac{n+3}{2}$ and $x=1-z^2$, and it will become

$$\frac{\frac{1}{2}\sqrt{1-a}\times\dot{x}}{\overline{1-x}\times\sqrt{ax+\frac{1-vx-\overline{1-x}|^{v}}{1-v}}}=\frac{1}{2}\,\overline{1-a}|^{\frac{1}{2}}$$ into

$$\overline{a-\frac{vx}{2}+\frac{v.\overline{v-2}}{2.3}\times x^2-\frac{v.\overline{v-2}.\overline{v-3}}{2.3.4}\times x^3\ \&c.}\Big|^{-\frac{1}{2}}\times$$

$$\overline{x^{-\frac{1}{2}}\dot{x}+x^{\frac{1}{2}}\dot{x}+x^{\frac{3}{2}}\dot{x}\ \&c.}$$

Now, to find the Fluent of the first Term hereof (drawn into the general Multiplicator) or $\overline{a-\frac{vx}{2}+\frac{v.\overline{v-2}}{2.3}\times x^2\ \&c.}\Big|^{-\frac{1}{2}}\times x^{-\frac{1}{2}}\dot{x}$, we have (as before) $n=1$, $m=-\frac{1}{2}$, $p=\frac{1}{2}$, $b=\frac{v}{2}$, $\frac{d}{b}=\frac{v-2}{3}$, $\frac{e}{b}=-\frac{\overline{v-2}.\overline{v-3}}{3.4}$, &c. Also $R=\frac{v-2}{6}$, $S=\frac{\overline{v-2}.\overline{4v-5}}{72}$; and consequently the Fluent itself (when the Body arrives at the lower Apse) $=\frac{P}{\sqrt{\frac{1}{2}v}}\times$

$$\overline{1+\frac{v-2}{2v}\times a+\frac{5.\overline{v-2}.\overline{4v-5}}{48v^2}\times a^2+\frac{7.\overline{2-v}.\overline{16v^2-37v+22}}{6.48v^3}}$$

&c. After the same manner the Fluent of the second

the second Term will come out $= \frac{p}{\sqrt{\frac{1}{2}v}} \times \overline{\frac{a}{v} + \frac{5.\overline{v-2}}{4v^2} \times a^2 + \frac{35.\overline{v-2}.\overline{2v-3}}{48v^3} \times a^3 \ \&c.}$ that of the third $= \frac{p}{\sqrt{\frac{1}{2}v}} \times \overline{\frac{5a^2}{2v^2} + \frac{35.\overline{v-2}}{12v^3} \times a^3}$ &c. &c. &c. Whence, by collecting these several Fluents together, we have $\frac{p}{\sqrt{\frac{1}{2}v}} \times \overline{1 + \frac{1}{2}a + \frac{20v^2-5v+2}{48v^2} \times a^2 + \frac{112v^3-63v^2+42v-8}{6.48v^3} \times a^3 \ \&c.}$ for the Fluent of the whole Expression: And this, drawn into $\frac{1}{2} \times \overline{1 - \frac{a}{2} - \frac{a^2}{8} \ \&c.}$ $(= \frac{1}{2} \times \overline{1-a}|^{\frac{1}{2}})$ will be

$$= \frac{p}{\sqrt{2v}} \times \overline{1 \ * \ \frac{\overline{v-2}.\overline{2v-1}}{48} \times \frac{a^2}{v^2} + \frac{\overline{v-2}.\overline{2v-1}|^2}{72} \times \frac{a^3}{v^3} \ \&c.}$$

$$= \frac{p}{\sqrt{n+3}} \times \overline{1 \ * \ \frac{\overline{n-1}.\overline{n+2}}{24} \times \frac{a^2}{\overline{n+3}|^2} + \frac{\overline{n-1}.\overline{n+2}|^2}{18} \times \frac{a^3}{\overline{n+3}|^3} \ \&c.}$$

which, in Degrees, gives $\frac{180}{\sqrt{n+3}} \times \overline{1 \ * \ + \frac{\overline{n-1}.\overline{n+2}}{24} \times \frac{a^2}{\overline{n+3}|^2} + \frac{\overline{n-1}.\overline{n+2}|^2}{18} \times \frac{a^3}{\overline{n+3}|^3} \ \&c.}$ for the true Measure of the Angle required.

XIII.

XII. *A Letter from Dr.* John Lining *to* C. Mortimer *M.D. Sec. R.S. concerning the Weather in* South-Carolina; *with Abstracts of the Tables of his* Meteorological Obſervations *in* Charles-Town.

SIR,

Read May 6. 1748.

THO' I have not the Pleaſure of a perſonal Acquaintance with you, yet as you are one of the Secretaries to the *Royal Society*, I take the Liberty to ſend you ſome Tables and Obſervations deduced from a Diary of the Weather, which I have kept for ſome Years paſt in this Town, which you may communicate to the *Royal Society*, if you think they will be acceptable. As an Account of the Inſtruments which I have uſed, and their Situation, is already publiſhed in the *Philoſophical Tranſactions* *, I ſhall not trouble you with a Repetition of thoſe Affairs.

The Viciſſitudes of the Weather, with reſpect to Heat and Cold, are perhaps no-where greater than in *Carolina*; and our Summer's Heat is probably not inferior to that under moſt Places of the Equator; nor is our Winter's Cold much leſs at ſome times than that in *Britain*.

From near eight Years Obſervation, the greateſt Increaſe of the Heat of the Air, which I have diſcover'd in 24 or 30 Hours, in Spring, Summer, Autumn, and Winter, was 19, 24, 13, and 16 Degrees

* Nº. 470, *p.* 497-8.

Degrees of *Fahrenheit*'s Thermometer; and the greatest Decreases of Heat, in the same Spaces of Time, in those Seasons, were 35, 32, 27 and [illegible] Degrees respectively. It frequently happens, that one Day is ten or more Degrees warmer than the preceding Day; but the Decreases of Heat are always greater and more sudden than its Increases. On the 10th of *January* 1745. at 2 *p. m.* the Mercury in the Thermometer was at 70; next Morning it had sunk to the 26th Degree; and on the 12th Day in the Morning it was at 15, which was the greatest and most sudden Change I have seen.

In Summer, the Heat of the shaded Air, about 2 or 3 in the Afternoon, is frequently between 90 and 95 Degrees; and on the 14th, 15th, and 16th of *June* 1738, at 3 *p. m.* it was 98; a Heat equal to the greatest Heat of the human Body in Health. In Winter I never but once saw the Thermometer so low as 15: Therefore the Difference between the most intense Heat and Cold of the shaded Air, in this Province, is 83 Degrees; which is a much greater Range than could well have been expected in this Latitude; and taking the Mean between those Extremes, 56 should be the temperate Degree of Heat in this Province: But the Sum of the thermometrical Altitudes, divided by the Number of Observations which I made for some Years together, gives 66, which may therefore more justly be reckoned the temperate Heat in *Carolina*, which exceeds 48, the temperate Heat in *England*, more than that exceeds the freezing Point.

The

The mean Heat of the ſhaded Air, in Spring, Summer, Autumn, and Winter, taken from the mean nocturnal Heat, and from the mean Heat at 2 or 3 *p. m.* is 61, 78, 71, and 52 Degrees.

The mean Heat of the ſhaded Air at 2 or 3 *p. m.* in Spring, Summer, Autumn, and Winter, is 65, 82, 75, 55 Degrees; and the mean nocturnal Heat in theſe Seaſons is 57, 74, 68, and 49 Degrees. Therefore our Winter's nocturnal Heat, at a Medium, coincides nearly with the temperate Heat in *England*.

The Thermometer, when ſuſpended five Feet from the Ground, and expoſed to the direct Rays of the Sun, and to thoſe reflected from our ſandy Streets, has frequently roſe in a few Minutes, from 15 to 26 Degrees, above what was at that time the Heat of the ſhaded Air (but I have never yet made that Experiment when the Heat of the ſhaded Air was above 88): When we are therefore expoſed in the Streets to the Sun in Summer, we inſpire Air from 4 to 28 Degrees warmer than the Heat of the human Body.

The Thermometer, when buried in the Sands of the Streets, when the Heat of the ſhaded Air was 88, roſe in 5 Minutes to 108, tho' there was at the ſame time a moderate Wind.

In *June* 1738, when the Heat of the ſhaded Air was 98, the Thermometer ſunk one Degree in my Arm-pits; but continued at 98 in my Hand and Mouth: From which we ſee what little Concern the Air has in cooling the Blood in the Lungs. Two Men who were then in the Streets (when the

Heat

Heat was probably 124 or 126 Degrees, as the ſhaded Air's Heat was then 98) dropp'd ſuddenly dead; and ſeveral Slaves in the Country, at Work in the Rice-Fields, ſhared the ſame Fate. I ſaw one of the Men immediately after he died; his Face, Neck, Breaſt, and Hands, were livid.

From the barometrical Table it appears, that the Barometer's mean Altitude, taken from its greateſt and leaſt Height, is 30.09 Inches; and that its Range is only 1.22 Inch. Wherefore our Atmoſphere varies only $\frac{1}{27}$ Part in its Weight. In the warm Months, the mean barometrical Station, taken from its greateſt and leaſt Altitudes in theſe Months, is 30.09 Inches; and I have never yet ſeen its Range in theſe Months exceed $\frac{58}{100}$ Parts of an Inch: Therefore the Changes of our Atmoſphere's Weight, in the warm Months, will have but little Effect upon human Conſtitutions, as the Difference between its greateſt and leaſt Preſſure is but $\frac{1}{5}$ Part of that in cold Climates, where the Range of the Barometer is three Inches. May not the great Height of the Barometer in the warm Months in this Climate, proceed from the vaſt Quantity of Water, which is at that time ſupported in our Atmoſphere, as the Exhalation is then very great; or may it not proceed from the Rarefaction of the Mercury? For the Weight of the mercurial Column, at equal Altitudes, will be different under different Degrees of Heat; and the Mercury may therefore be ſupported at equal Heights by Columns of Air of unequal Weights.

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It appears, from the barometrical Table, that our Easterly or northerly Winds elevate the Mercury, and that our Southerly or Westerly Winds depress it; and I have as yet never observed the contrary. I beg you will be good enough to excuse this long Epistle, and believe that it comes from one who has a sincere Regard to the Improvement of Natural Knowledge, and one who is,

SIR,

Your most humble Servant,

John Lining.

A TABLE

A TABLE *of the highest and lowest Stations of* Fahrenheit's *Mercurial Thermometer in the shaded Air, with the mean meridian and nocturnal Heat, taken after Dr.* Jurin's *Method.*

	1738		1739		1740		1742		1738		1739	
	Highest	Lowest	Highest	Lowest	Highest	Lowest	Highest	Lowest	Mean Altit. in the Heat of the Day	Mean Altit. about 10 p. m.	Mean Altit. in the Heat of the Day	Mean Altit. about 10 p. m.
January	71	36	69	19	67	30	68	34	60	53	53	45
February	72	34	75	38	75	27	68	25	57	51	63	55
March	74	32	79	40	80	34	77	32	62	56	65	59
April	86	50	84	50	83	51	88	59	75	65	69	65
May	91	52	86	62	87	56	88	63	79	70	77	73
June	98	59	87	69	90	66	90	65	87	77	81	74
July	91	70	89	68	91	70	95	73	85	77	82	75
August	89	64	87	60	90	67	93	60	82	75	82	75
September	83	62	88	59	84	56	86	43	76	71	71	68
October	74	41	79	43	73	35	78	42	65	59	68	60
November	70	37	69	30	67	32	66	32	57	51	57	51
December	67	28	70	30	69	21	69	34	53	50	59	53
The Means									70	63	69	66

	1740 Mean Altit. in the Heat of the Day	1740 Mean Altit. about 10 p. m.	1742 Mean Altit. in the Heat of the Day	1742 Mean Altit. about 10 p. m.	Mean Altitude in the Heat of the Day	Mean Altitude about 10 p. m.	Meridian Heat warmer than the Nights	Mean Heat taken from the Mean Meridian and nocturnal Heat	R. Society's Thermometer 1739 Highest	1739 Lowest	1740 Highest	1740 Lowest
January	49	43	55	49	54	48	6	51	40	74	44	67
February	59	49	52	45	58	50	8	54	36	60	40	69
March	63	54	60	53	62	50	6	59	35	63	34	65
April	74	65	77	67	74	60	8	70	32	55	33	55
May	78	70	79	70	78	71	7	75	31	46	31	50
June	83	74	83	72	84	74	10	79	31	42	28	44
July	86	76	86	79	85	77	8	81	29	42	27½	39
August	81	74	87	75	83	75	8	79	31	48	29	42
September	78	72	76	69	75	70	5	73	21	49	22	50
October	63	56	66	58	60	58	8	62	35	58	38	62½
November	56	50	52	46	50	50	6	53	42	67	42½	64
December	48	42	57	48	54	48	6	51	37	66	41	72
The Means	68	60	69	61	69	62	7	65½				

A

A TABLE *of the highest and lowest Barometrical Stations; with the Directions which the Wind then had.*

× A Northerly or Easterly Wind } preceded or succeeded.
♪ A Southerly or Westerly Wind }

Jan.						30.42	N	29.82	SW	.60
Feb.						30.38	NE	29.68	S	.70
March						30.26	SE	29.58	S	.68
April	30.42	E	29.42	W	.94	30.38	W×	29.78	WSW	.60
May	30.23	NE	29.85	S	.38	30.35	E	29.80	W	.55
June	30.20	NE	29.85	W	.35	30.30	E	29.98	SW	.32
July	30.13	SSW×	29.83	SW	.30	30.38	E	30.00	SW	.38
Aug.	30.18	E	29.88	SW	.30	30.38	NE	29.98	SW	.40
Sept.	30.33	NNE	29.85	SE	.48	30.38	E	29.88	NW	50
Oct.	30.33	E	29.82	WNW	.50	30.45	E	29.68	W	.77
Nov.	30.58	N	29.72	S	.86	30.35	NE	29.58	W	77
Dec.	30.60	N	29.93	W	67	30.58	N	29.75	NNW♪	.83

Jan.	30.70	N	29.50	NW♪	1.20	30.46	NNE	29.76	W	.70
Feb	30.55	N	29.85	W	.70	30.54	NNE	29.72	WSW	.82
March	30.50	SE	29.65	W	.85	30.40	ENE	29.60	W	.80
April	30.32	E	29.75	N♪	.57	30.48	E	29.58	W	.90
May	30.28	E	29.85	S	.43	30.30	S×	29.90	SSW	.40
June	30.18	S×	29.86	S	.32	30.28	ESE	29.90	NE♪	.38
July	30.08	SSE×	29.85	SSW	.23	30.22	W	29.98	SW	.24
Aug.	30.26	E	29.85	W	.41	30.25	NE	29.95	N	.30
Sept.	30.28	NE	29.85	NE♪	.43	30.36	NE	29.86	S	.50
Oct.	30.32	NNE	29.72	SW	.60	30.50	N	29.95	W	.55
Nov.	30.51	N	29.72	S	.79	30.55	NNW	29.73	SW	.82
Dec.	30.60	ENE	29.86	SW	.74	30.58	NNE	29.65	WNW	.93

 A

A TABLE *of the Depth of Rain, in Inches and millesimal Parts, which fell in* Charlestown.

	1738	1739	1740	1741	1742
January	1 097	2 310	4 873	4 492	2 189
February	4 416	2 875	3 084	4 615	1 650
March	4 532	5 609	1 141	5 713	5 203
April	1 082	0 195	1 092	1 308	0 918
May	3 127	5 120	5 612	4 841	5 898
June	1 567	15 839	4 648	5 538	3 250
July	10 660	5 452	3 013	3 399	1 252
August	4 104	12 211	7 301	7 144	7 647
September	10 792	4 834	3 200	6 734	2 895
October	1 358	6 593	1 258	3 399	0 759
November	2 656	1 235	1 848	2 964	3 388
December	3 877	3 689	2 736	1 919	0 957
Total Depth	49.268	65.962	39.806	52.066	36 006

	1743	1744	1745	The Means	1746
January	3 172	1 994	0 863	2 624	1 144
February	2 435	3 063	7 739	3 735	2 701
March	0 621	0 582	3 229	3 329	1 628
April	5 292	2 866	3 842	2 074	1 128
May	2 535	2 871	1 832	3 979	3 988
June	1 903	5 814	9 510	6 009	4 109
July	7 738	8 437	6 771	5 840	9 895
August	3 767	4 202	9 339	6 964	6 114
September	4 686	5 657	0 754	4 944	0. 932
October	1 672	1 595	2 962	2 450	
November	3 220	1 562	0 682	2 194	
December	2 706	9 680	2 623	3 523	
Total Depth	39.747	48. 323	50. 146	47.666	

XIII.

XIII. *An Abstract of the Bills of* Mortality *in* Bridge-Town *in* Barbados *for the Years* 1737 ——— 1744. *communicated by the Rev. Mr.* John Clark.

Read May 26. 1748.

Bridge-Town, Barbadoes.

An. Dom.	*Born*	*Males*	*Females*	*Baptiz'd*	*Buried*
1737	52	26	26	77	208
1738	81	41	40	106	250
1739	91	54	37	119	244
1740	91	49	42	123	242
1741	68	33	35	95	261
1742	87	42	45	130	296
1743	92	43	49	126	252
1744	89	46	43	120	166
	651	334	317	896	1919

XIV. *The Elements of a* Short Hand, *by* Samuel Jeake *Esq*;.

Read May 26. 1748.

A Succession of new Short-Hands published without the Reason of their Construction, having put me on forming a Method founded on Nature, the only Guide to Perfection, I settled an Alphabet in the following Manner.

Having

Having taken in a Book that lay by me a Paragraph as clear of the principal Idea of the Book as any I could find, conſiſting of near a thouſand Letters, I enumerated the Repetitions of each of them, and wrote them down; and thereby made the following Table of the Number of Times each Letter was repeated in 1000. 'Tis true, it cannot be ſaid the Repetitions will be exactly the ſame in every thouſand Letters that may be taken either in the ſame Book or another; but whoever will enumerate them will not find Difference enough to be of Conſequence.

The TABLE.

a	*b*	*c*	*d*	*e*	*f*	*g*	*h*	*i*	*k*	*l*	*m*	*n*	*o*	*p*
81	20	23	45	99	18	18	54	78	3	36	15	66	83	12
q	*r*	*s*	*t*	*u*	*w*	*x*	*y*	*z*.						
0	50	61	95	50	25	0	23	1.						

After having made this Table, I conſider'd with myſelf, that there were in Nature no more than eight ſimple Characters; four whereof are right, and the other four are crooked Lines.

The four right Lines are firſt the perpendicular Line |, and ſecondly the Line of Level —; which make the two Sides of a Square. Secondly the oblique Line / aſcending from the left to right, and the oblique Line \ deſcending from left to right, making the two Sides of the Rhomb; which is the Figure of the Diamonds on the Cards.

The four crooked Lines are only the Semicircle when the Diameter is either above or below it, or on the right or left Hand of it as, ∪ ∩ ⊂ ⊃.

All

4

All Characters whatever must be made up of these, and from their Composition, which introduces Ambiguity of Signification, arises the Difficulty of reading a Short Hand, which uses the simple Characters for some Letters, and compound Characters for other Letters; or, which is as bad, for Words.

This Difficulty, being unavoidable in a Short Hand of more than eight Letters, making it appear that 8 was the Number of Letters a short Hand ought not to exceed, I considered it in the following Light.

1. If *a*, *e*, *i*, *o*, and the Aspirate *h*, be suppress'd, there will be 19 Letters only remaining to be represented by 8 Marks.
2. If *c s x z*, which have a Sound much alike, be represented by one Character, there will remain 15 Letters to be represented by the other 7 Marks.
3. If *c g k q*, which have a Sound not very different, be represented by one Character, there will remain 12 Letters to be represented by 6 Marks.
4. If *b p f* be represented by one Mark, there will remain 9 Letters to be represented by 5 Marks.
5. If *d t* be represented by one Mark, only 7 Letters remain to be represented by 4 Marks.
6. If *l r* be represented by one Mark, only 5 Letters remain to be represented by 3 Marks.
7. If *m*, *n*, are represented by one Mark, only 3 Letters remain to be represented by 2 Marks.
8. If *u*, *w*, are represented by one Mark, there will remain one Mark to represent *y* the only Letter hitherto unmentioned.

Writing

Writing with Suppression of the Vowels hath been always admitted into short Hands of all sorts, because the Consonants are look'd upon as radical Letters, which indeed they ought to be. I shall suppress *h*, as being not radical.

All short Hands are subject to Ambiguity; for there being but 8 Marks to represent 24 Letters; and those 8 being used for 8 of them in the Short Hand Alphabets, the other Letters must be described by Characters compounded of these 8.

The ranging of the Letters into Classes, as is done here, will hardly introduce a greater Ambiguity than all short Hands are subject to. So that this Method cannot be reckoned more puzzling to a Reader than any of the rest.

1. The Repetitions of *d* being 45, and of *t* 95, amount to 140, for the Repetition of this Class.

2. The Repetitions of *l*, being 36, and *r* 50, amount to 86, for the Repetition of this Class.

3. The Repetitions of *m*, being 15, and *n* 66, amount to 86, for the Repetition of the third Class.

4. The Repetitions of *u*, being 50, and of *w* 25, give 75, for the Repetition of the fourth Class.

5. The Repetitions of *c*, when of the Nature of *s*, being about half its Number in the Table, may be reckoned 10, those of *s* 61, those of *x* 0, and those of *z* 1, give 72, for the Repetitions of the fifth.

6. The Repetitions of *b*, being 20, of *f* 18, and of *p* 12, give 50, for the Repetitions of the sixth Class.

7. The Repetitions of *c* before *a*, *o*, *u*, being about 13, of *g* 18, of *k* 3, and of *q* 0, give 34, for the Repetition of the 7th Clafs.

8. The Repetition of *y*, being 23, gives 23 for the Repetition of the 8th Clafs.

By a little Reflection it will appear, that the Marks applicable to thefe Claffes are in fome meafure determin'd. For a right Line taking up lefs Time than a crooked Line in its Defcription, it is plain the four firft Claffes muft be referred to the four right Lines; and the four circular Parts to the remaining four laft Claffes.

But the right Lines are indifferent to all the firft four Claffes, and the circular Parts to the four laft Claffes, for the Reafon juft mentioned. So that fo much as relates to the fixing the particular right Line to reprefent the particular Clafs, is at the Liberty of the Inventor of a Short-Hand, to adjuft agreeable to his own Fancy: And the fame is true of the circular Parts. Thus any one may perceive how far the Fancy of a Short-Hand Maker is properly bounded or at Liberty.

I fhall take notice of one fhortening Rule; which is that of increafing the Dimenfions of a Line, when the Letter muft be repeated fucceffively; as in *Man*, *rare*, and the like Cafes. This is a good Rule of Mr. *Wefton*.

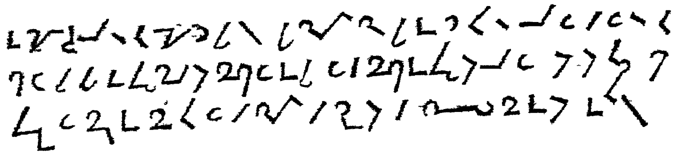
An Alphabet according to the Claffes.

dt. lr. mn. uw. csxz. bfp. cgkq. y

Z z

A

A Practise on the Lord's Prayer.



Which being expreſſed in Letters of the common Alphabet, will certainly convince the Reader how eaſily a Language may be read, tho' the Vowels are omitted. *e. g.*

ur ftr wc rt n vn, llwd b ty nm, ty kndm cm, ty wll b dn n rt
s t s n vn,
gv s ts dy ur dly brd, nd frgv s ur dts, s w frgv ur dtrs, nd ld s nt
nt tmptin, bt
dlvr s frm vl, fr tn s t kndm, t pwr, nd t glry, fr vr nd vr, mn.

As to the Introduction of Marks for repreſenting Words in a Short-Hand, I ſhall not now ſay any thing more about it, than that all Short-Hands, this as well as any other, is equally ſuſceptible of them.

The Advantages of this Short-Hand in the State exhibited, when perfectly learned, ſo as to be wrote readily, will appear to be,

1. That, by Suppreſſion of *aeioh*, or $\frac{400}{1000}$, only $\frac{6}{10}$ of the Time of writing ordinary Long-Hand is neceſſary to write this.

2. That the ſimple Strokes repreſenting the Conſonants, not taking up above half the Time of writing the Conſonants, only half of $\frac{6}{10}$, or $\frac{3}{10}$ of the Time of any thing wrote in Long-Hand is neceſſary for writing this.

3.

3. Right Lines not taking up more than $\frac{2}{3}$ of the Time of Description of crooked Lines, as the Diameter is $\frac{2}{3}$ of the Semiperiphery, it appears, if only right Lines were used, these $\frac{3}{10}$ would be reduced to $\frac{2}{10}$, by the Subtraction of $\frac{1}{3}$ of $\frac{3}{10}$. But, because the Number of right Lines, all things consider'd, should not be reckon'd but about double the Number of crooked ones, only $\frac{2}{3}$ of $\frac{1}{10}$ can be taken from the $\frac{3}{10}$; that is to say, the Time taken up in writing this Hand will be $\frac{9}{30} - \frac{2}{30} = \frac{7}{30}$ of the Time taken up in writing of the common Long-Hand, or or less than the $\frac{1}{4}$ of the Time.

As I have shewn all the Principles on which Short-Hands can be constructed to Advantage, I have no need to compare this with any other; because I have enabled every Reader to judge of them, by shewing within what Limits all Improvements are bounded.

XV. *An Account of a Treatise by* Wm. Brownrigg *M.D. F.R.S. intituled,* "The "Art of making common Salt, as now "practised in most Parts of the World; "with several Improvements proposed in "that Art, for the Use of the *British* Do- "minions;" *abstracted by* W. Watson *F. R. S.*

Gentlemen,

Read June 15. 1748.

I RECEIVED your Commands to lay before you an Extract of our worthy Brother Dr. *Brownrigg*'s Book; which, though at

all times ready to execute whatever you think proper to charge me with to the utmoſt of my Abilities, I engaged in the more readily, from the Pleaſure and Inſtruction I had already received from the Peruſal of that excellent Work, in which its Author has eminently diſtinguiſhed himſelf both as a Chemiſt, and as a Philoſopher.

This Work conſiſts of 295 Pages in 8vo, excluſive of the Preface, and of 6 Copper-Plates, exhibiting different Views of Salt-Houſes, Inſtruments, *&c.* neceſſary to the Preparation of Salt. It is enriched likewiſe with Notes of great Importance to the Work, not only of the Author, but alſo from the *Philoſophical Tranſactions*, *Medical Eſſays*, *Memoirs of the Royal Academy of Sciences at Paris*, *Pliny*, *Agricola*, *Alonſo Barba*, *Ramuſio*, *Boyle*, *Hoffman*, *Liſter*, *Herrera*, *Dampier*, *Baccius*, *Pomet*, *Marſilli*, *Plot*, *Scheuchzer*, *Hales*, *Raſtel*, *Leigh*, *Boerhaave*, *Shaw*, and others.

Amongſt the vulgar Arts, that of preparing Sea-Salt for the Uſes of Mankind hath been thought worthy the notice of many great and learned Men, as well antient as modern. Thus many things relating to this Art are recorded by *Cato* and *Pliny*, *Agricola* and *Hoffman*, to whom our Author is much indebted for thoſe Memoirs that have been tranſmitted to us, relating to its Hiſtory. Had thoſe great Men been as diligent in improving this Art, as they were in recording the Improvements made therein by others, there would not now have been Occaſion to remark, that, after the Practice of ſo many Ages, an Art ſo ſimple, and withal ſo neceſſary, hath not yet been brought to any Degree of Perfection.

That

That this Art was capable of great Improvements, eſpecially as practiſed in *Great Britain*, was the Sentiment of this *Society* ſoon after its Inſtitution; at which time the Members thereof were very intent upon bringing it to a greater Perfection; as may be gather'd from the Inquiries and Suggeſtions of Dr. *Beal*, and the Hiſtories of ſeveral Methods of making Salt, which then were publiſhed by the *Society*. And although the *Engliſh* have, ſince that time, conſiderably improved their Method of boiling Salt; yet this Art is ſtill practiſed with greater Skill and Succeſs by the *Dutch*, as the ſuperior Goodneſs of the Fiſh, cured with their Salt, doth ſufficiently prove.

The Commons of *Great Britain*, having taken into Conſideration the great Importance of this Art, judged ſome Improvements propoſed therein worthy their Regard and Encouragement; well knowing, that, could this be brought to the ſame Perfection in *Britain* as in ſome neighbouring Countries, large Sums of Money might be ſaved in the Nation, which are now paid to the *French* and others; its Fiſheries improved, and its Navies and Commerce, and many of its richeſt Colonies, would no longer depend upon its Enemies for one of thoſe Neceſſaries, without which they cannot be ſupported.

Theſe Conſiderations have induced our Author to give a brief Account of the various Methods of making Salt, which are now uſed in *Great Britain*, and in other Countries, where this Art is practiſed with more Succeſs; and alſo to attempt ſeveral further Improvements for the Uſe of the *Britiſh* Dominions. How far he has ſucceeded in theſe Attempts,

tempts, will best appear, if the Public shall think the following Proposals so far worthy their Attention, as to merit a fair and impartial Trial. The principal Conclusions, deduced from a Variety of Observations and Experiments, are as follows: 1. That, by the Methods here proposed, an excellent Bay-Salt may be made in *Britain* in very large Quantities, so as to be afforded cheaper than at the Prices paid for foreign Salt; and that the *British* Colonies in *America* may very commodiously be supplied with Bay-Salt of their own Manufacture, without having recourse for it to the *French*, *Spaniards*, and *Portuguese*. 2. That, by the Methods here proposed, an excellent kind of refined white Salt may be made in *Britain*, as well from Sea-Water and Rock-Salt, as from natural Brine, in any Quantity wanted, so as to be afforded cheaper than foreign Bay-Salt; and which will also be better for curing Fish, Flesh, and other Provisions.

In forming these Conclusions, an impartial Regard has been had to Truth, without attending to the private Advantage of any particular Set of Men. The Sense of this, together with a Desire of promoting the public Advantage, has induced our Author to communicate the following Sheets at this time, although by deferring the Publication some time longer he might have made them possibly more accurate; because, besides other Considerations of no small Import, an Opinion has prevailed, that the establishing of Fisheries in the North of *Scotland* would be the best Means of affording an useful Employment to more unciviliz'd Inhabitants of that Part of the Kingdom, for carrying on of which they are most commodiously situated.

What

What Mr. *Lowndes* * hath lately done towards the Improvement of Brine-Salt, may, perhaps by some, be thought to supersede the Necessity of further Attempts for improving and extending our Salt Manufacture. Dr. *Brownrigg* is very far from depreciating the Endeavours of that Gentleman, which have met with Parliamentary Encouragement; and had his Discovery appeared to the Doctor sufficiently complete and extensive, he would not have given the Public and himself this Trouble. He makes no Doubt but that the Specimen of Salt, which Mr. *Lowndes* exhibited before the College of Physicians, was a strong and pure Salt, since such it appeared to that most learned Body. Whether the Alum mixed with it (agreeable to the antient Practice of the *Cheshire* Salt boilers) contributed any thing to its Goodness, is more properly consider'd hereafter. It is only necessary here to observe, in Justification of the present Undertaking, that Mr. *Lowndes*'s Method of making Salt for curing Provisions, doth not appear to be the best that may be put in Practice; since our Author hopes to shew, that, by other Methods, a purer and a stronger Salt may be made, and at a less Expence. Neither is his Method so general and extensive as seems to be required for the public Good; since Mr. *Lowndes* confines it almost intirely to boiled Brine-Salt; and hath given no Directions concerning the Preparation of Bay-Salt. He indeed proposes to meliorate the *British* Sea-Salt, but seems to despair of preparing a Salt either from Sea-Water, or *English* Rock-Salt, fit

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* Mr. *Lowndes*'s Process is inserted in this Work. See *p.* 104 *et seq.*

fit for the Uſes of the Navy or Fiſheries; altho' the *Dutch* Salt, which is the ſtrongeſt and pureſt boiled Salt now made, is entirely a marine Salt, and even the Brine, of which Mr. *Lowndes* makes his Salt, is only a Solution of the *Engliſh* Rock-Salt, often in very impure Water, as is well known to the Naturaliſts.

Our Author, treating of Salt in general, takes notice of the Excellence and Uſefulneſs thereof; and that it hath pleaſed the *Author* of Nature to provide Mankind therewith in ſuch Abundance, that there are few Countries which do not afford vaſt Quantities of Rock or foſſil Salt. Mines of it have been long diſcovered and wrought in *England, Spain, Italy, Germany, Hungary, Poland*, and other Countries in *Europe*. Moreover the Sea affords ſuch vaſt Plenty thereof, that all Mankind might thence be ſupplied with Quantities ſufficient for their Occaſions. There are alſo innumerable Springs, Ponds, Lakes, and Rivers impregnated with common Salt, from which the Inhabitants of many Countries are plentifully ſupplied herewith.

In ſome Countries, which are remote from the Sea, and have little Commerce, and which are not bleſſed with Mines of Salt, or ſalt Waters, the Neceſſities of the Inhabitants have forced them to invent a Method of extracting their common Salt from the Aſhes of Vegetables.

In ſhort, this Salt is diſperſed all over Nature; it is treaſured up in the Bowels of the Earth; it impregnates the Ocean; it deſcends in * Rains; it fertilizes

* See *Boyle* on the Saltneſs of the Sea.

tilizes the Soil; it ariſes in Vegetables; and from them is convey'd into Animals; ſo that it may well be eſteemed the univerſal Condiment of Nature.

Naturaliſts, obſerving the great Variety of Forms under which this Salt appears, have thought fit to rank the ſeveral Kinds of it under certain general Claſſes, diſtinguiſhing it moſt uſually into Rock or foſſil Salt, Sea-Salt, and Brine or Fountain-Salt: To which may be added others of thoſe muriatic Salts, which are found in vegetable or animal Subſtances. Theſe ſeveral Kinds of common Salt often differ from each other in their outward Form and Appearance, or in ſuch accidental Properties as they derive from the heterogeneous Subſtances with which they are mixed; but, when perfectly pure, they have all the ſame Qualities; ſo that Chemiſts, by the exacteſt Inquiries, have not been able to diſcover any eſſential Difference between them. In this our Author agrees with the celebrated † *Hoffman*. Leaving therefore theſe Diviſions to thoſe whom they may concern, it may for the preſent Purpoſe be more proper to diſtinguiſh common Salt after a different Manner into the three following Kinds; *viz.* into Rock or native Salt, Bay-Salt, and white Salt. By

* *Hoffman* de ſalinibus *Hallenſ.* cap. viii.

Ut igitur noſtra hac de re innoteſcat ſententia, hanc interponimus; ſicuti in tota univerſi hujus orbis compage, una tantum eſt aqua, unus per fermentationem paratus ſpiritus ardens, unus Mercurius, unum volatile ſal, unum acidum nitroſum ac vitriolicum ſal; ita, pari ratione unum idemque ſal commune eſt. Sed quum plures alienæ, terreæ, lapidoſæ, ſulphureæ, calcariæ minerales ac pingues particulæ cum hiſce corporibus connubium ineant, diverſa exinde emergit eorum indoles; et ſal commune idem ſemper obtineret ingenium, ſiquis pingues terreas, calcareaſque partes ab illo artificioſe ſegregaret.

By Rock-Salt *, or native Salt, is underſtood all Salt dug out of the Earth, which hath not undergone any artificial Preparation.

Under the Title of Bay-Salt may be ranked all Kinds of common Salt extracted from the Water, wherein it is diſſolved by means of the Sun's Heat, and the Operation of the Air; whether the Water, from which it is extracted, be Sea-Water, or natural Brine drawn from Wells and Springs, or Salt Water ſtagnating in Ponds and Lakes.

Under the Title of white Salt, or boiled Salt, may be included all Kinds of common Salt extracted by Coction from the Water wherein it was diſſolved; whether this Water be Sea-Water, or the ſalt Water of Wells, Fountains, Lakes, or Rivers; or Water of any ſort impregnated with Rock-Salt, or other Kinds of common Salt.

The firſt of theſe Kinds of Salt is in ſeveral Countries found ſo pure, that it ſerves for moſt domeſtic Uſes, without any previous Preparation, Triture excepted. But the *Engliſh* foſſil Salt is unfit for the Uſes of the Kitchen, until by Solution and Coction it is freed from ſeveral Impurities, and reduced to white Salt. The *Britiſh* white Salt alſo is not ſo proper as ſeveral Kinds of Bay-Salt for curing Fiſh, and ſuch Fleſh-Meats as are intended for Sea Proviſions, or for Exportation into hot Countries. So that, for theſe Purpoſes, we are obliged, either wholly

* By *Rock-Salt*, or *Sal Rupium*, the antient *Chemiſts* mean Salt adhering to the Rocks above the high Water Mark, being there lodged by the Spray of the Sea, evaporated by the Heat of the Sun; which is the pureſt Salt of all for chemical Uſes, and is to be had off the Rocks of *Sicily*, and ſeveral Iſlands in the *Weſt Indies*.

C. Mortimer.

wholly or in part, to uſe Bay Salt, which we purchaſe in *France*, *Spain*, and other foreign Countries. To remedy theſe Inconveniencies this Treatiſe was wrote, in order to ſhew how the Subjects of *Great Britain* may be ſupplied with Salt of their own Manufacture, fit and ſufficient for all their Occaſions.

In order that the Methods here propoſed might be better underſtood, and that the Reaſonableneſs of them might more fully appear, the Author thought it neceſſary to premiſe a brief Account of the ſeveral Ways of preparing Bay-Salt, as well as white Salt, as far as they came to his Knowledge. From this Hiſtory may be formed a Judgment, how far the Methods now in Uſe are proper, in what deficient, where erroneous, and how they may be improved.

Bay-Salt in general may be divided into two Kinds. Firſt, Bay-Salt, drawn from Sea-Water, as is practiſed in *France*, *Spain*, *Portugal*, and many other Countries. Secondly, Bay-Salt extracted from ſalt Springs, Ponds, and Lakes; as at *Cape de Verd* Iſlands, *Tortuga*, and other Places. Of theſe the firſt is imported in large Quantities into *Great Britain* and *Ireland*: Our *American* Colonies, in Times of Peace, are chiefly ſupplied with the latter; but in Time of War they have large Quantities of Bay-Salt from *Lisbon*, and other Parts of *Portugal*.

Bay-Salt is prepared in a Manner the moſt ſimple and eaſy, when the Water of Ponds and Lakes impregnated with Salt is totally exhaled by the Force of the Sun and Air, and the Salt is left concreted into a hard Cruſt at the Bottom of the Lake or Pond. Of Salt thus prepar'd we have Inſtances

in many Parts of the World, as in the *Podolian* Desert near the River *Borysthenes* on the *Russian* Frontiers towards *Crim Tartary*, in the Kingdom of *Algiers*, and in other Parts of the World.

Bay-Salt is also drawn from the Brine of Ponds and Lakes, and our Author gives us an Account of the preparing it in this manner in the *Cape de Verd* Islands. This Account was collected chiefly from the Relations of several Persons of Credit, who themselves assisted in making Salt in these Islands. He also takes notice of the Bay-Salt made at *Tortugas*, and other Places in *America*. He describes likewise the Manner of making marine Bay-Salt in *France*, and other Parts of *Europe* For the Particulars of these Operations I must refer you to the Work itself; and only take notice, that every kind of Bay-Salt is prepared without artificial Heat, and by only exposing the Brine under a large Surface to the Action of the Sun and Air, by which, in proportion to the Strength of the Brine, and to the different Temperature of Climate and Season, the Salt chrystallizes into what we call Bay-Salt, and comes under different Appearances to us from different Places, which arise principally from the Cleanliness and Care of the Artist.

Our Author, when treating of white Salt in general, acquaints us, that although Salt is made, in warm Climates, with the greatest Ease, and at the least Expence, by the Heat of the Sun, after the Methods already described; yet, in several Countries, where Bay-Salt might be conveniently made, they prepare all their Salt by culinary Fires. Thus in *Austria*, *Bavaria*, and many other Parts of *Germany*, and also in *Hungary*, and even in some Parts of *Italy*, they constantly boil the Water of

their ſalt Springs into white Salt. But in other Parts of *Europe*, as in *Britain*, and in the Northern Parts of *France* and *Germany*, an erroneous Opinion long prevailed, that the Heat of the Sun was not there ſufficiently intenſe, even in the Summer Seaſon, to reduce Sea-Water, or Brine, into Bay-Salt. And all Arguments would probably have been inſufficient to remove this Prejudice for the *Engliſh*, had not the contrary been fully proved by Experiments, which were firſt accidentally made in *Hampſhire*. However, the Method of making Salt by Coction will probably ſtill continue to be practiſed in *Britain*; as the Salt ſo prepared is for ſeveral Uſes preferable to Bay-Salt; and when prepared after a particular Manner, is preferable to common Bay-Salt, even for curing Proviſions, as the Practice of the *Hollanders* ſufficiently teſtifies: So that the due and right Preparation of white Salt ſeems very deſerving of the Notice and Regard of the Public.

White Salt, as it is prepared from various ſaline Liquors, may therefore be diſtinguiſhed into the following Kinds:

1. Marine boiled Salt, which is extracted from Sea-Water by Coction. 2. Brine or Fountain-Salt, prepared by Coction from natural Brine, whether of Ponds or Fountains. 3. That prepared from Sea-Water, or any other kind of Salt-Water, firſt heightened into a ſtrong Brine by the Heat of the Sun, and the Operation of the Air. 4. That prepared from a ſtrong Brine or *Lixivium* drawn from Earths, Sands, or Stones impregnated with common Salt. 5. Refined Rock-Salt, which is boiled from a Solution of foſſil Salt in Sea-Water, or any other kind of ſalt Water, or pure Water. 6. Laſtly, Salt upon Salt, which is Bay-Salt diſſolved in Sea-Water,

or

or any other ſalt Water, and with it boiled into white Salt; and under theſe Heads may be ranked the ſeveral kinds of boiled Salt now in Uſe. Our Author has given us an exact Hiſtory of the Manner of preparing theſe different kinds of Salt, as practiſed in different Places, with miſcellaneous Obſervations and Cautions relating to their reſpective Proceſſes, for which in the general I muſt refer you to the Work itſelf: But the making Salt upon Salt deſerves more particular Attention; as the Author, being under no Tie of Secrecy, has revealed to us the Method of making in *Holland* and *Zealand* that ſtrong and pure kind of Salt, with which they cure Herrings, and all other Proviſions for long keeping; which gives the *Dutch* a great Advantage over all other Nations in the Herring-Fiſhery; ſince Fiſh preſerved with this Salt look much cleaner and fairer than thoſe that are cured with Bay-Salt, and keep much better than thoſe preſerved with any other kind of white Salt.

From the Proceſs whereby white Salt is made from Sea-Water by Coction, it appears, that Sea-Water, beſides common Salt, contains ſeveral other Ingredients; ſome of which are ſeparated before the common Salt falls, and others remain in the Bittern, after all the Salt is extracted. Our Author has given a full and circumſtantial Account of theſe in an expreſs Chapter, under the Appellation of Memoirs for an Analyſis of Sea-Water.

The Salt-Boilers, and particularly thoſe who prepare Brine-Salt, have long been accuſtomed to make uſe of various Subſtances, which they call Additions or Seaſonings, and mix them with the Brine while it

is

is boiling, either when they firſt obſerve the Salt begin to form, or elſe afterwards during the Time of Granulation. Theſe Additions they uſe for various Purpoſes. Firſt, to make the Salt grain better, or more quickly form into Chryſtals. Secondly, to make it of a ſmall fine Grain. Thirdly, to make it of a large firm and hard Grain, and leſs apt to imbibe the Moiſture of the Air. Fourthly, to render it more pure. And laſtly, to make it ſtronger, and fitter for preſerving Proviſions.

Theſe Additions, moſt commonly uſed to anſwer the above-mention'd Purpoſes, are Wheat-Flour, Reſin, Butter, Tallow, new Ale, ſtale Beer, Bottoms or Lees of Ale and Beer, Wine-Lees and Alum. Wheat-Flower and Reſin are uſed for the Property they poſſeſs of making the Salt a ſmall Grain. Butter, Tallow, and other unctuous Bodies are commonly applied, as they are ſaid to make the Brine chryſtallize more readily; for which End ſome Salt-Boilers more particularly prefer the Fat of Dogs: But others have little to plead for their uſing theſe Subſtances, but immemorial Cuſtom: How far they have the Effects aſcribed to them can only be determined by Experiments, as ſeveral Boilers, who formerly uſed them, now find they can make as good Salt without them. Wine-Lees, new Ale, ſtale Ale, the Lees of Ale and Beer are now generally rejected by the marine Salt-Boilers; except in the Weſt of *England*, where the Briners, who uſe them, affirm that they raiſe a large Grain, and make their Salt more hard and firm, and ſome ſay that they make it chryſtallize more readily. *Hoffman* prefers the ſtrongeſt Ale; and *Plot* aſſures us, that it

it makes the Salt of a larger or smaller Grain, according to the Degree of its Staleness. The only good Effects that fermented Liquors can have as an Addition, are probably owing to their acid Spirit, which may correct the alcaline Salts of the Brine, and so render the common Salt more dry and hard, and less apt to dissolve in moist Air. If therefore it should be thought necessary to use any of these Additions, in order to correct the alcaline Quality of the Brine, stale Ale, or *Rhenish* Wine *, ought to be chosen, as new Ale contains but little Acid.

Alum is an Addition long known and used in *Cheshire*, together with Butter, to make the Salt precipitate from some Sorts of Brine, as we are assured by Dr. *Leigh* in his Natural History of *Lancashire*, *Cheshire*, &c. who first taught the *Cheshire* Salt-Boilers the Art of refining Rock-Salt. As the bad Properties of their Salt proceeded from hard boiling, they found every Method ineffectual, until they had recourse to a more mild and gentle Heat. And as Alum hath been long disused amongst them, it is not likely, that they found any extraordinary Benefit from it; otherwise they would scarce have neglected it, and continued the Use of Butter. However Mr. *Lowndes* hath lately endeavour'd to revive its Use; asserting, that Brine-Salt hath evermore two main Defects, Flakyness and Softness; and to remedy these Imperfections, he tried Alum, which fully answered every thing he proposed; for it restored the Salt to its natural cubical Shoot, and gave it a proper Hardness; nor had it any bad Effect whatever. But our Author is of Opinion, that whoever considers the Nature of Alum, will scarce expect

* Why not Malt-Vinegar? C. M.

pect ſuch extraordinary Effects from it. Neither does it here ſeem wanted; for the Grains of common Salt will always be ſufficiently hard, and of their natural Figure, large Size, and no-ways diſpoſed to run by the Moiſture of the Air, if formed by a gentle Heat, and perfectly free from heterogeneous Mixtures: So that the Goodneſs of Mr. *Lowndes's* Salt does not ſeem owing to the Alum, with which it is mixed, but chiefly to the gentle Heat uſed in its Preparation.

The *Dutch*, who have long ſhewn the greateſt Skill and Dexterity in the Art of boiling Salt, make uſe of another Addition, which they eſteem the greateſt Secret of their Art. This is Whey, kept ſeveral Years till it is extremely acid; now firſt revealed by our Author to the *Britiſh* Salt-Boilers, but long held in great Eſteem by the *Dutch*, for the good Effects it hath upon their Salt; which it renders ſtronger, more durable, and fitter to preſerve Herrings, and other Proviſions.

Bay-Salt, as well as white Salt, is of different Kinds, and poſſeſſed of different Qualities: With the different Kinds of theſe Proviſions muſt be cured, according to the Uſes for which they are deſigned. The *Dutch* indeed uſe no Salt for curing Proviſions, beſides their own refined Salt. With it they can preſerve Fleſh and Fiſh of all Kinds as well as with the ſtrongeſt Bay-Salt; and chuſe to be at the Expence of refining Bay Salt, rather than to defile their Proviſions with the Dirt and other Impurities, with which it commonly abounds.

Salt, eſteemed the beſt for curing Proviſions, and for preſerving them the longeſt time, is that which is the ſtrongeſt and the pureſt. This may be known by

 the

the following Characteristicks; *viz.* it is usually concreted into large Grains or Chrystals, which are firm and hard, and in respect to those of other Kinds of common Salt, the most solid and ponderous; it is not disposed to grow moist in a moderately dry Air, to which it has been exposed a considerable time; its Colour is white, and somewhat diaphanous; it hath no Smell; its Taste is truly muriatic, and more sharp and pungent than that of other Kinds of common Salt. It has, besides these, several other distinguishing Properties mentioned by our Author. The Salts, which approach nearest to this Degree of Perfection, are the best Kinds of Bay-Salt, and the strong *Dutch* refined Salt; but most of the Salt now made for Sale is very far from answering to these Characteristics.

Having related the various Methods of preparing Salt that now are in Use, as far as they are come to our Author's Knowledge, it appears, that this Art is not brought to such Perfection in the *British* Dominions as in several other Countries, the Salt there prepared being unfit for preserving many Kinds of Provisions. It remains now to shew, that this Want of a strong Salt of *British* Manufacture proceeds not from any Defect in Nature, but of Art; and that, if proper Skill and Industry be used in the *British* Dominions, and due Encouragement there given by the Legislature, such Improvements may be made in this Art, that not only *Great Britain*, but *Ireland* also, and the *British* Colonies in *America*, may be supplied with Salt of their own Manufacture, proper for curing all Kinds of Provisions, in Quantity sufficient for all their Occasions, in Quality equal, if not superior, to any foreign

Salt

Salt now made, and at a moderate Price. These are Truths, which the Author hopes will appear evident from the Facts and Reasonings contained under the following Positions:

Lemma I. The Quantity of Water which annually falls in Rain, Snow, and Hail, is very different in different Parts of *Great Britain*; there commonly falling almost double the Quantity on the Western Coasts, that falls on the Eastern Coasts of that Island.

Lemma II. The Quantity of Rain which falls in *Lancashire*, during the four hottest Months of the Year, *viz. May*, *June*, *July*, and *August*, doth not at a Medium amount to more than a third Part of the Quantity of Water, which falls in Rain, Snows, and Hail, during the whole Year.

Lemma III. The Water which ascends in Vapours from the Sea very greatly exceeds that which descends thereon in Rain and other aqueous Meteors: But the Quantity of Water, which usually exhales from a given Part of the Ocean in a given Time, cannot with any Exactness be determined.

Lemma IV. The Quantity of Water which commonly exhales in *Great Britain* from shallow Ponds during the four hottest Months of the Year, greatly exceeds the Quantity of Rain which commonly falls on the Surface of those Ponds during the said Months.

From these *Lemmata*, which the Author has supported by the Observations, not only of himself, but of other learned Men, are deduced the following Propositions:

Proposition I. In ſeveral Parts of *England* large Quantities of Bay-Salt may be extracted from Sea-Water during the hotteſt Months of the Year, by receiving the Salt-Water into Ponds, and ſuffering its aqueous Parts thence to exhale by the Heat of the Sun, and the Operation of the Air and Winds.

Prop. II. In ſeveral Parts of *England* large Quantities of Bay-Salt may very commodiouſly be extracted from Sea-Water, after the ſame manner that is practiſed in *France*, and in other Parts of *Europe*.

Prop. III. Bay-Salt may be extracted in *England* from Sea-Water in larger Quantities, and with more Certainty, than by the foregoing Method, if Care be taken to preſerve the Brine contained in the Salt-Pits from being diluted with Rains, and to promote the Evaporation of the Water by ſeveral artificial means, which may eaſily be put in Practice.

Prop. IV. In ſeveral Parts of *England* large Quantities of excellent Bay-Salt may with great Eaſe be made from the natural Brine of ſalt Springs, and alſo from Rock-Salt diſſolved in weak Brine or Sea-Water.

Prop. V. Bay-Salt may be prepared in *England* by the foregoing Methods at a very moderate Expence, equal in Goodneſs to the beſt foreign Bay-Salt, and in Quantity ſufficient for the Conſumption of all the *British* Dominions.

Prop. VI. In ſeveral of the *British* Colonies in *America*, Bay Salt might, with little Expence and Trouble, be prepared from Sea-Water, in Quantities ſufficient to ſupply the *American* Fiſheries,

and

and all other Occaſions of thoſe Colonies, ſo as to become a conſiderable Branch of their Trade.

The Author has ſupported all theſe Propoſitions with great Ingenuity; but I cannot paſs over in Silence the artificial means to promote the Evaporation of Sea-Water, mention'd in *Prop.* III. as well as to preſerve the Brine contained in the Salt-Pits from being diluted with Rains. I therefore ſhall lay before you a ſhort Account of theſe.

It will be proper, ſays our Author, to make all the Salt-Pits of the Marſh in one long Row extended from Eaſt to Weſt, and for each Pit to make Covers of thin Boards, or rather of coarſe Canvas, or Sail-Cloth, ſtretched on Frames of Wood and painted white. Theſe Covers muſt all be fixed with Hinges to ſtrong Poſts and Beams on the North Side of the Pits; ſo that they may be let down and drawn up with Cords and Pulleys, or by ſome other Contrivance, ſomewhat like Drawbridges. Theſe Covers thus fixed may be let down over the Pits like a Shed or Penthouſe in rainy Weather; and in dry Weather may be erected almoſt to a a Perpendicular, but inclining a little towards the South; ſo as to form a Wall with a South Aſpect. Thus theſe may ſerve a double Purpoſe, as Coverings for the Pits in wet Weather, and as Reflectors of the Sun's Heat upon them in dry Weather, and thus greatly promote the Evaporation of the aqueous Parts of the Brine. The Hinges on which the Reflectors turn may be fixed about eight or ten Inches from the Ground; by which means, when the Reflectors ſtand upright, there will be an Opening left beneath them, through which the Air will continu-

ally

ally flow in a brisk Current, and greatly increase the Evaporation of the Water.

After having gone through that Part of Dr. *Brownrigg*'s Work, which relates to Bay-Salt, we proceed to the Methods that Gentleman proposes for preparing and improving white Salt, which, if brought into Use, may probably be of Advantage not only to private Undertakers, but also to the Public. For it appears, that two very different Kinds of white Salt are required; the one for the Use of the Table, and the other as a Condiment for Provisions. Its Whiteness, Dryness, and the Smallness of its Grain, are the Properties which chiefly recommend the first Kind; and its great Strength and Purity the latter. It is this strong and pure Kind of white Salt, which is wanted in the *British* Dominions; and it is therefore our Author's principal Design here to consider how this Defect may be supplied; although at the same time Instructions are given how to prepare Table Salt, not only better in Quality, but also at a less Expence than it is now prepared by the common Methods.

Lemma I. In the common Processes for making white Salt, the Salt is deprived of a considerable Part of its acid Spirit, by the violent Boiling used in its Preparation.

Lemma II. Most Kinds of white Salt are render'd impure by the Mixture of various heterogeneous Substances.

Lemma III. White Salt, by the violent Coction commonly used in its Preparation, is render'd less fit for preserving Fish, Flesh, and other Provisions, than it would be if prepared with a more gentle Heat.

Lemma

Lemma IV. The heterogeneous Subſtances which are commonly mixed with white Salt, render it leſs proper for preſerving Proviſions, than it would be if ſeparated from them.

After having fully conſider'd the foregoing, our Author gives a Method of preparing a Kind of white Salt proper for curing Fiſh, Fleſh, and other Proviſions; likewiſe a Method of refining Salt; but for theſe I muſt refer you to the Work itſelf, as well as for the Tables, wherein the ſeveral Expences attending theſe Operations are minutely conſider'd.

Moſt of the Facts referred to in theſe Diſquiſitions are ſuch, as the conſtant Practice of thoſe who make Salt ſufficiently warrants us to rely upon for true and certain; or elſe, they are the Obſervations of judicious Salt-Officers, daily converſant in theſe Matters, or of curious and inquiſitive Navigators, Merchants, Travellers, and Naturaliſts; or, laſtly, the Experiments of many learned Phyſicians, Chemiſts, and Philoſophers: The Truth of which ſeveral Facts, though many of them have long been publiſhed, hath never been called in Queſtion. So that theſe Obſervations and Experiments may probably be more relied on by the Public, than if they had only been made by our Author; ſince they have the Teſtimony of many skilful and unprejudiced Perſons, who could have no Notion of the Uſes to which they have been here applied. If therefore the Arguments founded upon thoſe Facts ſhould be eſteemed any-ways reaſonable and ſatisfactory, the Author preſumes to remark, that it might not be unworthy the Wiſdom of the *Britiſh* Legiſlature to direct

direct a more full Inquiry to be made into a Matter of this Importance, and to order proper Works to be erected for making Bay-Salt, and for making and refining white Salt, and to put those Works under the Management of able and judicious Persons, to make exact and accurate Trials, in order to discover the best and cheapest Methods of doing them. And the Methods, which should be most approved of, might for the general Good be made public, and established by Law as a common Standard, to which all those who make Salt in the *British* Dominions should be obliged to conform.

However imperfect this Extract may appear, I must now beg your Indulgence for having taken up more of your Time than is usually allow'd to Works of this kind. I must plead in my Excuse the great, the National Importance of the Work itself, the masterly Manner with which the Subject-Matter is treated, as well as its falling in so exactly with that Institution, in which we are so desirous of distinguishing ourselves. The making and refining Salt must certainly be considered as one of those mechanic Arts, the History of which, as we are taught by the noble * *Verulam*, is a necessary Part of that Knowledge, that true Science of Nature, which is not taken up in vain and fruitless Speculations, but effectually labours to relieve the Necessities of human Life.

XVI.

* *Verulam de Aug. Scient.* lib. II. cap. 2.

XVI. *A Catalogue of the* Immerſions *and* Emerſions *of the* Satellites *of* Jupiter, *that will happen in the Year* 1750, *of which there are* 173 *of the Firſt,* 85 *of the Second,* 94 *of the Third, and none of the Fourth, by reaſon of its great Latitude; in all* 322. *Computed to the Meridian of* London *from the* Flamſteedian *Tables: Corrected by* James Hodgſon *F. R. S. Maſter of the Royal Mathematical School in* Chriſt's-Hoſpital.

Note, Thoſe that are marked with an Aſteriſk are viſible at *London*.

THE great Improvements made in the uſeful Sciences of Geography and Hydrography, by the Obſervations of the Eclipſes of *Jupiter*'s Satellites, are too well known to need any Account of them, or Encomium upon them. They were judged very proper for this Purpoſe by *Galilæus* himſelf, who firſt diſcovered them, and all the Aſtronomers at that time, as being the moſt certain, ſure, and eaſieſt Method then known, and I may venture to ſay even to this Time, for aſcertaining the Difference of Longitudes between Places, how remote ſoever: And it is this that has encouraged Perſons to make conſtant Obſervations of them: And that they may not neglect the fre-

quent Opportunities that offer themselves for want of timely Notice, I formerly published in the *Philosophical Transactions* * an annual Catalogue of all the Eclipses that would happen that Year, as I do now for the succeeding Year, and shall continue to do for the Time to come, if my Health will permit me: And if those Persons who shall make any Observations, will be so good as to communicate them to me, they will be gratefully received, as they will tend to discover the Errors of the Tables, which shall be my constant Endeavour to find out, so long as it shall please the divine Providence to enable me to do it.

ECLIPSES *of the first Satellite of* JUPITER.

Emersions.				Emersions.				Emersions.			
D.	H.	M.	S.	D.	H.	M.	S.	D.	H.	M.	S.
JANUARY.				21	8	42	16*	8	1	29	26
				23	3	10	58	9	19	58	13
1	21	30	11	24	21	19	49	11	14	27	26
3	15	58	11	26	6	8	20*	13	8	56	45
5	10	26	13	28	10	36	57	15	3	26	1
7	4	54	32*	30	5	5	34*	16	21	55	13
8	23	23	7	31	23	34	19	18	16	24	25
10	17	51	43					20	10	53	39
12	12	20	21	FEBRUARY.				22	5	22	17*
14	6	48	38*					23	23	52	17
16	1	17	5	2	18	2	59				
17	19	45	28	4	12	31	50				
19	14	13	50	6	7	0	39*	APRIL.			

* See N°. 449, in the Year 1738. and others in the preceding Years.

ECLIPSES *of the first Satellite of* JUPITER.

Immersions.

D.	H.	M.	S.
APRIL.			
25	2	13	23
26	20	42	15
28	15	10	54
30	9	39	33
MAY.			
2	4	8	17
3	22	36	52
5	17	5	26
7	11	33	57*
9	6	2	27
11	0	30	59
12	18	59	24
14	13	27	49
16	7	56	14
18	2	24	53
19	20	52	50
21	15	21	4*
23	9	39	27
25	4	17	40
26	22	55	53
28	17	14	7
30	11	42	21
JUNE.			
1	6	10	22
3	0	38	33
4	19	6	44
6	13	34	56*
8	8	3	6
10	2	31	17
11	20	59	25
13	15	27	38*
15	9	55	57*
17	4	24	13
18	22	52	18
20	17	20	31
22	11	48	42*
24	6	16	54
26	0	44	7
27	19	13	25
29	13	41	39*
JULY.			
1	8	9	56
3	2	37	36
4	21	6	56
6	15	33	20*
8	10	2	43
10	4	31	8
11	22	59	40
13	17	28	12
15	11	56	44*
17	6	25	18
19	0	53	53
20	19	22	33
22	13	51	19
24	8	20	5
26	2	48	51
27	21	16	39
29	5	46	26
31	10	15	17*
AUGUST.			
2	4	44	8
3	23	11	38
5	17	40	38
7	12	9	38*
9	6	38	39
11	1	7	41
12	19	36	44
14	14	5	53*
16	8	35	2

ECLIPSES *of the first Satellite of* JUPITER.

Immerſions.				Immerſions.				Emerſions.			
D.	H.	M.	S.	D.	H.	M.	S.	D.	H.	M.	S.
18	3	4	14	29	14	44	45	4	2	29	4
19	21	33	26					5	20	57	35
21	16	2	58	OCTOBER.				7	15	26	1*
23	10	31	55*	1	9	13	58*	9	9	54	24*
25	5	0	54	3	4	42	52	11	4	22	45
26	23	30	56	4	22	12	3	12	22	24	6
28	17	59	42	6	16	41	4*	14	17	19	25*
30	12	29	3*	8	13	18	8*	16	11	47	43*
				10	5	39	2*	18	6	16	1*
SEPTEMBER.				12	0	8	0	20	0	44	11
1	6	58	24					21	19	12	21*
3	1	27	47	Emerſions.				23	13	40	35*
4	19	57	4	13	20	45	9	25	8	8	47*
6	14	27	40*	15	15	13	59*	27	2	36	58
8	8	55	32*	17	9	42	51*	28	21	4	59
10	3	24	38	19	4	11	39	30	15	33	1*
11	21	53	22	20	22	40	26				
13	16	22	17*	22	17	9	13*	DECEMBER.			
15	10	51	21*	24	11	37	53*	2	10	1	37*
17	5	20	41	26	6	6	33*	4	4	58	59*
18	23	49	52	28	0	35	12	5	22	57	0
20	18	19	54	29	19	3	46	7	17	24	58*
22	12	48	13*	31	13	22	1*	9	11	52	58*
24	7	17	21*					11	5	20	55*
26	1	46	29	NOVEMBER.				13	0	49	18
27	20	15	36	2	8	0	35*	14	19	16	57*

Eclipses *of the first Satellite of* Jupiter.

Emersions.				Emersions.				Emersions.			
D.	H.	M.	S.	D.	H.	M.	S.	D.	H.	M.	S.
16	13	45	0*	21	21	9	10	27	4	33	32*
18	8	13	3*	23	15	37	15*	28	23	1	41
20	2	41	9	25	10	5	24*	30	17	29	44*

Eclipses *of the second Satellite of* Jupiter.

Emersions.			
D.	H.	M.	S.
January.			
4	7	4	13*
7	20	21	36
11	9	39	41
15	22	57	19
18	26	15	13
22	1	33	4
25	14	51	24
29	4	9	33*
February.			
1	17	28	3
5	6	46	29*
8	20	4	57
12	9	24	50

Emersions.			
D.	H.	M.	S.
15	22	43	8
19	12	1	36
23	1	22	2
☉	♃	☌	
Immersions. April.			
24	9	11	17
27	22	29	24
May.			
1	11	47	54
5	1	5	55
8	14	23	58
12	3	41	45

Immersions.			
D.	H.	M.	S.
15	16	59	43*
19	5	17	23
22	18	34	48
26	8	52	26
29	23	9	36
June.			
2	11	27	0
6	0	44	27
9	14	1	41*
13	3	54	57
16	16	36	10*
20	5	53	40
23	19	10	55
27	8	28	19*
30	21	45	48

July.

ECLIPSES *of the second Satellite of* JUPITER.

Immersions.

D.	H.	M.	S.
JULY.			
4	11	3	21
8	0	20	33
11	13	38	46*
15	2	56	55
18	16	14	40
22	5	22	54
25	18	51	15
29	8	9	39*
AUGUST.			
1	21	28	13
5	10	46	52*
9	0	6	31
12	13	25	20*
16	2	43	20
19	16	2	27*
23	5	21	42
26	18	40	56
30	8	0	13*
SEPTEMBER.			
1	21	19	25

Immersions.

D.	H.	M.	S.
6	10	29	4*
10	2	58	18
13	13	18	8*
17	2	36	28
20	15	55	57*
24	5	16	2
27	18	34	9
OCTOBER.			
1	7	53	17*
4	20	52	20
8	10	31	19*
11	23	50	14
Emersions.			
15	15	41	38*
19	5	0	12
22	18	18	42*
26	7	36	58*
NOVEMBER.			
2	10	13	28*

Emersions.

D.	H.	M.	S.
5	23	30	21
9	12	48	34*
13	2	6	15
16	15	33	40*
20	4	40	52
23	17	58	3*
27	7	15	22*
30	20	32	11
DECEMBER.			
4	9	38	48*
7	23	5	35
11	12	22	26*
15	1	39	23
18	14	56	15*
22	4	13	19
25	16	30	23*
29	6	47	30*

JANUARY.

ECLIPSES *of the third Satellite of* JUPITER.

D.	H.	M.	S.		
JANUARY.					
3	15	59	8	I	
	18	43	40	E	
10	19	59	35	I	
	22	43	5	E	
17	23	58	38	I	
18	2	42	6	E	
25	6	42	35	E	*
FEBRUARY.					
1	10	42	49	E	
8	19	43	54	E	
15	18	43	26	E	
22	22	49	20	E	
APRIL.					
21	4	58	50	I	
28	8	50	37	I	
MAY.					
5	12	49	50	I	*
12	16	48	53	I	
19	21	18	22	I	
27	1	35	16	I	
	3	11	18	E	
JUNE.					
3	4	42	45	I	
	7	7	57	E	
10	8	39	52	I	
	11	4	38	E	
17	12	38	38	I	*
	15	2	58	E	*
24	16	34	27	I	
	18	58	11	E	
JULY.					
1	20	31	55	I	
	22	55	21	E	
9	1	30	11	I	
	3	52	23	E	
16	4	28	18	I	
	6	50	16	E	
23	8	28	22	I	*
	10	48	6	E	*

AUGUST.

ECLIPSES *of the third Satellite of* JUPITER.

D.	H.	M.	S.		
AUGUST.					
6	16	50	33	I	*
	19	8	50	E	
13	20	52	57	I	
	23	10	59	E	
21	0	56	33	I	
	3	13	5	E	
28	4	59	26	I	
SEPTEMBER.					
4	8	53	10	I	*
11	13	36	30	I	*
18	17	9	37	I	*
25	21	13	2	I	
OCTOBER.					
3	1	16	18	I	
10	5	19	12	I	*
17	11	29	55	E	*
24	15	36	29	E	*
31	19	37	27	E	
NOVEMBER.					
7	23	37	36	E	
15	3	37	30	E	
22	5	22	40	I	*
	7	34	22	E	
29	9	24	2	I	*
	11	35	22	E	*
DECEMBER.					
6	13	22	28	I	*
	15	33	24	E	*
13	17	20	18	I	*
	19	31	2	E	
20	21	17	21	I	
	23	27	53	E	
28	1	16	50	I	
	3	27	10	E	

Now, inafmuch as, in the Beginning of this Year, the Latitude of the fourth Satellite is greater than the Breadth of the Shadow of *Jupiter*, the Satellite will pafs wide of it, and there will be no

Eclipſe of it till the Middle of *June* in the Year 1752.

It is to be obſerved, that, for about a Month before, and a Month after the Conjunction of *Jupiter* with the Sun, by reaſon of the Proximity of *Jupiter* to the Sun, the Eclipſes cannot be obſerved. And this is the Reaſon that no Notice has been taken of them in the Catalogue between the 24th of *February* and the 25th of *April* following.

The Times here ſet down are according to the aſtronomical Way of reckoning, which ſuppoſes the Day to commence at the Noon of each Day, or when the Sun is upon the Meridian; and counting the Time on in a ſucceſſive Order, without the Diſtinction of Morning and Afternoon, till the Sun returns to the Meridian again the next Day at Noon. Thus, for Example, in the preceding Catalogue, the firſt Emerſion of the firſt Satellite is ſaid to happen *January* 1. at 21 Hours 21 Minutes and 11 Seconds; that is, according to the Civil Way of reckoning, on *Jan.* 2. at 30 Minutes 11 Seconds after 9 in the Morning.

An *Addition* to Dr. *Hales's* Paper, *p.* 279. by *C. Mortimer*.

TWO Days after the Fire-Works had been play'd off in the *Green Park* on account of the late Peace, I went all over the Building erected for that Purpose, and was greatly pleased to see the Doctor's Scheme confirmed by the Practice of the Engineers upon that Occasion; for the Room, in which the Trains were fired, and which was immediately under the Gratings upon which the 6000 Rockets rested and were fired from, had the Floor cover'd over with fine sifted Gravel about an Inch deep, and the Walls were whited over with a dirty sort of white Wash, which I took for Lime finely powder'd, and mix'd up with Size and Water, and done two or three times over. Both Floor and Wall were of Deal.

Printed for C. DAVIS, over-against *Gray's Inn Gate* in *Holbourn*, PRINTER to the ROYAL SOCIETY, M.DCC.XLVIII.

Numb. 488.

PHILOSOPHICAL TRANSACTIONS.

For the Month of *June*, 1748.

VI.

The CONTENTS.

I. *A Letter from the Rev.* Henry Miles *D. D. F. R. S. to the* President, *concerning the* Storm *of* Thunder, *which happen'd* June 12. 1748.

S I R,

Read June 23. 1748.

I BEG Leave to communicate to you and to the *Royal Society* some Account of the Effects of a Thunder-Storm, on two adjoining Houses, at *Stretham* in *Surrey*, the 12th Instant, a little after 5 in the Morning.

The preceding Day had been remarkably hot, and in the Afternoon very cloudy, with the usual Indications of an approaching Storm, in the Evening. At 9 at Night, the Wind Southerly, my Barometer stood thus; one, which is the most sensible, at 29 Inches $\frac{7}{10}\ \frac{3}{100}$; the other at 30. $\frac{2}{10}$ 0. The Thermometers (of *Sisson*'s Construction) one without-doors, at 43 Degrees, another within, at 49 Degrees above 0. or the freezing Point.

At one next Morning, a Person apprehensive of the Thunder, upon looking out at Window, was surprized to find an unusual clear Sky, every-where equal to what is observed in frosty Weather, or after a high Wind, except that in a few Places some Thunder-Clouds shewed themselves just above the Horizon.

At 2 we heard Thunder at a Distance: At half an Hour past 3, when I got up, I perceived the Storm approaching apace from the South, where the Wind then was, but the darker Clouds seemed to bear off

chiefly to the Eaſt and Weſt of us, ſo that I did not think we ſhould hear of any Miſchief near us. At 4 we had a ſmart Shower of Rain, and about 5 two loud Claps of Thunder over our Heads, but pretty high; the Lightning was very pale, and the Flaſhes large, deſcending in a ſpiral Form, almoſt perpendicular to the Horizon to the Eaſtward of us which is the Situation of *Stretham*, and at about 2 Miles diſtant from us. At a little before my Barometers ſtood thus, 29. $\frac{7}{10}$ $\frac{3}{100}$, and 30. $\frac{2}{10}$ $\frac{2}{100}$; and continued ſucceſſively riſing and falling during the Storm, but very inconſiderably.

Upon hearing two Houſes were damaged, ſituate at the Foot of the Hill on which the mineral Wells are, fronting the Eaſt, by the Wood-Side, I went next Day to view them. The Houſe to the South, which is a public Houſe kept by Mr. *Howard*, ſeem'd to have received the greateſt Shock. Some of the Family being up, the front Door ſtood partly open, when the Storm began: The upper half was of Glaſs, fram'd like a Saſh-Window, having two ſliding Shutters, one on each Side, which had not been taken down. The Glaſs between them was ſhatter'd to Pieces, but the Shutters no-ways touch'd, except that a Nail in one of them was forc'd in a little way. To the Door-Poſt, on the left Hand, hung by an iron Pin an iron Bar, which ſerved to faſten the Door at Night: This Pin was driven out of the Poſt, and the Bar conſiderably bent, and in divers Places melted in ſmall Spots, as were the Hinges of the Door, chiefly upon the Edges in both, and the Door-Poſt ſplit. A Sheet of Lead on the Pediment, or Shelter over the ſaid Door, was raiſed, and partly rolled

up

up at one Corner; the Cornice underneath being torn off without being ſplit, a good Part of the Tileing near the Eaves and over the Pediment was looſened, and ſome Tiles beat off, and the Lathing and ſome of the Moldings of the Windows had taken Fire.

In a Bed-Chamber fronting the Road, on the ſecond Floor where Mr. *Howard* lay, three Boards of the Lining of the Room, on the Eaſt Side, were driven inwards five or ſix Inches at one End; but at the other the Nails were a little looſened only. In a Garret over this Bed-Chamber, the upper Part of a Bed-Poſt was ſhiver'd; and nearly over where this Bed ſtood, a large Hole was broke in the Roof, on the Weſt Side, juſt by where one of the Chimneys goes up; the Chimneys having all additional Funnels of Brick-Work on the Top, of a roundiſh Form, and plaſter'd: Theſe were ſtruck, and inclin'd to the North, eſpecially that which was on the South End of the Houſe, the Plaſter being beat off, and ſome of the Bricks broke down. There were about 13 Perſons in this Houſe, none of which received any Hurt; tho' a Lad, who was in the Kitchen, into which the Door open'd, before-mention'd, and the Window of which (near where he was ſtanding) had ſeveral Panes of Glaſs broke, muſt certainly be much expoſed. He inform'd me, among other things, that the Fire flew about him in Sparks, like thoſe which fly out of burning Charcoal, but larger, and ſnapping as they do. Some Pieces of Glaſs were ſhewed me, which I found to have been melted, one of which I take the Liberty of laying before you.

 The

The adjoining Houſe, inhabited by Mr. *Figgins*, had the Plaſtering beat off in the Front in Patches, and one of the Chimneys crack'd for a great Length. In the Kitchen Window-Frame, one of the croſs Pieces, near the middle of the Window, had a Chip ſtruck off from it about 5 Inches in Length, and at one End about a Quarter of an Inch thick, but thin at the other, and near the Width of the Frame, but none of the Glaſs broke, nor the Lead bent, tho' in a manner contiguous with the Splinter beat off. The ſame thing happen'd to a Parlour-Window, on the other End of the Houſe; both the Shivers were found directly oppoſite to the Windows, at ten or twelve Yards diſtant in the Road.

In a ſmall Garret (which is next to Mr. *Howard's* Houſe) where two Maid-Servants lay, the Plaiſter was broken, to Appearance, inwards, on oppoſite Sides of the Room, and near the Feet of the Bed, which ſtood on each Side about three Quarters of a Yard from the Wall. The Breach on the Eaſt Side, near a Window (ſome Panes of the Glaſs of which were broken) was oppoſite to the Vailings of the Bed, which were ſinged, and a Hole burnt thro' them big enough to receive the End of one's fore Finger. On the oppoſite Side, juſt by the Chimney, another Breach was made, of the ſame Height, in the Wall, which was continued downwards for about a Yard, but the Curtains not at all ſinged. Directly againſt this Breach, one of the Maids (who had got up) ſat on the Bed's Side, who was inſtantly ſtruck down, but received no Hurt: Upon enquiring of her, whether ſhe ſeemed to receive a Blow on

any particular Part of her Body? ſhe replied, ſhe was ſtruck all over alike.

But the moſt remarkable, tho' the leaſt terrible Effect, appeared on the Frame of a Pannel of Wainſcot, about five Feet long, and about one and a half wide, in the Parlour fronting the Eaſt: On this Pannel a Landſcape is painted, and the Moulding belonging to it had been gilt, but on the laſt painting the Room, the Gilding was cover'd with the ſame Paint: That which cover'd the gilt Moulding was ſtripped off in irregular ragged Streaks throughout, ſo that the Gilding appeared as freſh as it may be thought to have look'd when it was painted at firſt: And as the Gilding does not ſeem to have been affected, ſo neither does the Paint appear to have been crack'd any-where, but where the Gilding lay under.

If it be ſuppoſed, that the Lead in the Paint was melted by the Lightning, it will be difficult to account for it, that it ſhould *not at all affect* the Paint contiguous with that which was upon the Gilding; tho' we ſuppoſe a Reſiſtance to have been made by the Leaf-Gold, and to have contributed to the producing the mention'd Effects. But fearing I have been already too prolix, I ask Leave to ſubſcribe myſelf, with the greateſt Reſpect,

SIR,

Your and the Royal Society's

Tooting, June 23. 1748. *moſt humble and moſt obedient Servant,*

H. Miles.

II.

II. *A Letter from* John Byrom *M. A. F. R. S. to the* President, *containing some Remarks on Mr.* Jeake's *Plan for* Short-Hand.

Honoured Sir,

Read June 23. 1748.

AS it has been suggested to me, that I should take some notice of the Plan for a Short-Hand by *S. Jeake* Esquire, which was lately * read before us at the *Royal Society,* I take the Liberty of addressing to you the following Remarks upon it; being obliged to thank you for the public Testimony which you were pleas'd to give, on that Occasion, in favour of the Method which I had the Pleasure of communicating to you; and which, in your Judgment, confirm'd by the Experience of many other Gentlemen who have learn'd it, appears sufficiently to be perfected to Demonstration.

In the Paper read before us it is inferr'd from the continual Succession of new Short-Hands, that none of them were constructed upon right Principles, which, in the Opinion of the Proposer of this Plan, are briefly these;

1. There are in Nature but 8 simple Characters, *viz.* 4 rectilinear ones, (| — / \ ) and 4 crooked or semicircular (⌒ ◡ ⊃ ⊂).

2. To avoid the Ambiguity and Confusion that must arise from the Use of compound Characters, a perfect Short-Hand should consist of these 8 simple ones only.

3. But whereas there are 3 times as many Letters (or more) in the common Alphabet, the Conse-quence

* See *Phil. Trans.* n. 487. p. 345.

quence is, that one Character muſt ſerve for one, two, three, or four Letters; as their Frequency of Occurrence, or Affinity to each other, ſhall ſuggeſt.

4. From theſe Suppoſitions, amongſt a Variety of Alphabets that would equally anſwer his Intention, reſults the following, which (omitting, as needleſs, the Letters *a, e, i, o, h*) he propoſes for the Plan of a perfect Short-Hand; and computes, with great Exactneſs, that it may be written in leſs than one Quarter of the Time that common Long-Hand will require.

The Alphabet.

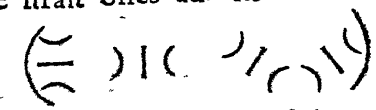
dt. lr. mn. uw. csxz. bfp. cgkq. y.

This, Sir, with a Specimen of the Lord's Prayer, as written in it, is the Whole of his Plan; which, as far as it goes, might have a plauſible Appearance to a Gentleman, at the firſt Turn of his Thought towards Short-Hand; but a little practical Attention muſt have ſhown him how liable it was to the very Objection that he intended to remedy, *viz.* Ambiguity.

The firſt Mark, for inſtance (L), in this ſhort Specimen, ſtands for theſe 4 ſeveral Words which occur in it, *viz. our, will, evil, ever*; and forty more that one might enumerate, muſt, whenever they occur, be repreſented by it; not to mention how often it muſt occaſion Ambiguity in the Beginning, Middle, or End of a longer Word, or Marks, whereof it is a conſtituent Part only.

Now, though in the Lord's Prayer it is eaſy, or in caſual Writing one of his Learning and Saga-

city might be able, by a long Familiarity with the Characters, to determine the Senſe of what was written in them, yet it is evident, that, to common Learners, a Difficulty ſo perpetually occurring muſt appear inſuperable.

The Poſtulatum, likewiſe, which this Plan for Short-Hand is grounded on, is taken up too expeditiouſly; for, there being, in Nature, 4 rectilinear Strokes, the horizontal, the perpendicular, and the acute, and grave (if I may ſo call them); it is manifeſt, by Inſpection, that from theſe 4 Directions there will ariſe, at leaſt, 8 curvilinear Characters, as each of the ſtrait ones admits diſtinctly of 2 oppoſite Curves  and there is no abſolute Neceſſity that any of them ſhould be always ſemicircular; a Shape that, for the moſt commodious Combination of ſimple Characters, is in fact much oftener inconvenient than otherwiſe.

The Alphabet then of ſimple Characters may be fairly enlarged by one third; and Room be alſo left for the Fancy of an Inventor to extend it farther, if he ſhould find it convenient upon the Whole.

I ſay, upon the Whole; for the worſt Short-Hand may happen to expreſs a few particular Words better than the beſt; and arbitrary Marks for Words or Sentences may be often ſhorter than regular ones: But this is no Inducement to write, in one Caſe, by a bad Method, and in the other, by none at all.

Another Overſight, in this Plan, is the Neglect of Beauty and Linearity; though the Simplicity of

its Characters does not, perhaps, admit of such enormous Scrawling as others may.

For, to instance again in the Specimen; suppose the Mark for the Word Temptation  which expresses a vast Variety of different Combinations of Consonants, to be limited, by a previous Knowledge of the Language, to that Word only, yet, after all, it is a very aukward one; and ought, by a common Short-Hand Rule of leaving out such Consonants as are not sounded (as the *p* is not in *Temptation*) to have been form'd in another Manner () wherein the Beauty and Linearity, and, of course, the Brevity of the Mark would have been preserv'd.

But Emendations of this Nature would, I doubt, in many Cases, which continued writing upon this Hypothesis must exhibit, be utterly impracticable.

In short, this Gentleman set out upon right Principles, which many hap-hazard Undertakers have but little consider'd; but he had not Leisure enough, perhaps, to examine them to the Bottom; as was the Case with Dr. *Green* of *Cambridge* (he that wrote the *Greenian Philosophy*, as he calls it), who form'd a Short-Hand for his own private Use, upon much the same Plan and Principles. He gave me one of his Sermons in it; and, upon Suggestion of the Advantages that he might have taken, he said, that for want of Time to consider of his Scheme more thoroughly, when he first adopted it, he had overlook'd them.

A perfect Short-Hand, I suppose, would be a Solution of some such Problem as this: —— "A Language being given, to assign the most compendious

 "Method

"Method of expressing it readily, and legibly, by an "Alphabet, and Rules, the best adapted to that Purpose."

How easy soever the general Principles of such a Method may appear to be, there is a deal of Nicety required to put them in Execution: An exact Attention to continual Trials and Amendments is necessary to ascertain the Preference amongst an infinite Variety of Dispositions, which Inventors may select, and throw their Characters into. So that it is no Wonder, that so many Publishers of new Short-Hands content themselves merely with Newness, or at most with some still imperfect Meliorations of foregoing Attempts.

This Gentleman proceeds no farther than to make an Alphabet for his Plan *; but must be sensible, that, were it never so complete a one, many compendious Applications of it might be obtain'd by a proper Enquiry into the Nature of our Language (the most happily susceptible of this Art of any) and the Abbreviations which it admits of, very intelligibly, in Writing.

And, in his Alphabet, he entirely omits the Letter *h* (which is often wanted), and the Vowels *a*, *e*, *i*, *o*, and yet retains the Vowel *u*, which is certainly as needless as any of the rest: But as a single Point, in five distinct Situations, would have provided for them all alike, he might as well have added that to his Plan, in order to express any particular Vowel, upon Occasion; because it would not

* Mr. *Jeake* only offers his Plan as the mere Elements of a Short-hand, leaving it to every Practitioner to build upon his Foundation, as they shall judge necessary from Practice: He retains the *u*, because it often stands for *v* or *ve* or *w*. *C. M.*

not have hurt his Alphabet; and becaufe the Reading of his Short-Hand without any Vowels at all, is fo extremely difficult.

For, as one of his ftrait Strokes (\ ) muft ftand for the Words *am*, *an*, *in*, *on*, *no*, *me*, *him*, *home*, *&c.* and one of his crooked ones (c) for *as*, *is*, *us*, *fo*, *has*, *his*, *eafe*, *ice*, *ufe*, *ax*, *ox*, &c. and fo of the reft; he would himfelf, in all Probability, be often at a Lofs to diftinguifh what he had written, upon his own Plan *.

The Confonants *j* and *v* he has taken no notice of; as if the common Way of repeating 24 Letters did really give a juft Idea of an Alphabet; which it does not; nor can a perfect Short-Hand for our Language (or any other refpectively) well be plann'd, without confidering the real Alphabet, or Table of every particular Sound, or Modification of Sound; that is to fay, Vowel or Confonant which occurs in it; and then adjufting the proper Characters to them, and taking all the Advantages that either Nature or Cuftom may afford.

I do not recollect, that any Author has ever enter'd into fuch a Difquifition with a View to Short-Hand; but, with a View to other Advantages, many different Accounts have been given of a real or univerfal Alphabet; feveral of them by eminent Members of the *Royal Society*: That propos'd by Mr. *Lodwick*, in particular, is publifh'd in the *Tranf.* N°. 182. but might, I think, be reduc'd into lefs Compafs,

* Vowels may be known to be antecedent or confequent, by the Mark being wrote above or below the Line of Level: *e g.* c *as*, c *fa*; \ *am*, \ *ma*: The Ambiguities in many of thefe Words are not important, *viz.* *as*, *has*, *is*, *his*, *ufe*, *us*. *S. J.*

Compaſs, and ſet in a plainer Light; and if it be agreeable, I ſhall, on this Occaſion attempt to do it.

In the mean time, Sir, it will not, I hope, be thought impertinent in me to offer theſe Remarks upon the Plan that has been laid before us, of an Art which I have taken ſo much Pains to cultivate, and bring to that Perfection which my firſt and laſt Intention of introducing one common Standard, for the general Practice of it, requir'd.

If I have ſucceeded, Gentlemen will, I perſuade myſelf, concur to facilitate the Deſign; which tho' the Accidents of Life, at a Diſtance from this Place, have hitherto retarded, I am intent upon accompliſhing to the utmoſt of my Power.

I thank you, Sir, for the many Marks of Friendſhip which you have ſhown me, and am

Your obliged humble Servant,

J. Byrom.

III. *Part of two Letters from Mr.* B. Cooke *F.R.S. to Mr.* Peter Collinſon *F.R.S. concerning the ſparkling of* Flanel, *and the* Hair *of* Animals *in the dark.*

I.

Newport, Iſle of Wight, May 19. 1748.

Dear Couſin,

Read June 23. 1748.

I FANCY at laſt this Sparkling of the Flanel *, and ſuch-like Bodies, will be found to be quite electrical: And it is poſſible, I conceive

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* See *Phil. Tranſ. n.* 483, *p.* 457.

conceive, that the acid Steams of the Sulphur, burnt under the extended Flanel in the Time of bleaching, may unite themselves with the Oil (with which Hair, as well as Horns, are found by Analysis to be replete), and form an animal Sulphur, which, upon Friction, Vibration, or any nimble Agitation of these Hairs, may become luminous.

And that something like this may be in the Case seems not improbable; since it hath been observed, that this Appearance hath happened most conspicuous in frosty Weather; in which Season there is generally not only a greater Purity of the Air, and Absence of Moisture; but all hairy and horny Substances (and Hairs you know are but small Horns) are more elastic, and consequently susceptible of, and capable of exciting, the strongest Vibrations. And, on the contrary, the lixivial Salts used in washing may destroy the sulphureous Acid, and discharge the Oil; whence the Hairs will become more flexible and limber, and be rendered less fit for exciting the electrical Fire. And the same may happen when Flanel is much worn, and by that means filled with the alcaline *Effluvia*'s, which go off from most (of the higher Order of) Animals by Transpiration; which may dissolve the animal Sulphur, weaken the Spring of the Hairs, and so render the Phænomenon more difficult. I am,

My good Friend,

Most sincerely yours,

Benj. Cooke.

II.

Dear Cousin, *Newport*, June 1. 1748.

IT ſhould have been mention'd, that the Flanel had been worn but few Days; and that it was immediately upon ſhaking the Under-Coat from that which was worn above it, that the Sparks were emitted; and that their Appearance was in a broad Streak almoſt contiguous, attended with a Crackling or Snapping, like what may be obſerved on moving the Finger nimbly along over the prime Conductor, when excited in the electrifying Machine; of which the Lady was able to form a Compariſon, having afterwards ſeen ſome Experiments of that ſort.

This Appearance returned at the ſame time, and on the ſame Occaſion, two or three Nights after, but more languid, till it was quite loſt.

A Lady, who was informed of this, leſſened the Surprize (which had been thought almoſt ominous) by aſſuring, that ſhe had ſeen the ſame Phænomenon often in new Flanel, but never in any that had been long worn or waſh'd: And that the Flanel being render'd damp with Sea-Water, and afterwards dried, would heighten the Flaſhing which ſhe imputed to the Sulphur us'd in bleaching. However that be, I ſhall only obſerve, that theſe Sparklings had the crackling Criterion of electrical Fire; and that Hair and Wool, as well as Silk, are Electrics *per ſe*, and unctuous and ſulphurous Bodies more electric than others of the ſame Denſity.

Dr. *Wall* hath oblig'd the Public with a curious Diſſertation on a ſimilar Subject, which I gueſs would be

be particularly entertaining while you are on this Speculation. See *Phil. Transf.* N°. 314, *p.* 69.

Bartholin ſuppoſes unctuous *Effluvia* to have a great Share in theſe Appearances: His Words are theſe, which I chuſe to quote; the Book * *de Luce Animalium* being not very common: " Imo quod " admirationem excedit, collectæ oleaginoſi effluvii " reliquiæ, longo interjecto tempore, in ſcintillas " reſolvuntur: ſi enim faſcias vel tænias ſerico textas, " ſed uſu detritas, leviter excutiamus, igniculi ſuſci- " tantur et ſcintillæ:"--- and quotes a Paſſage out of *Geſner de Herbis lucentibus*, to confirm his Opinion.

The ſame Writer tells us, that *Theodore Beza* was to be ſeen in the dark, " ob fulgorem externum " circa oculorum orbes;" —— but whether this Light proceeded from the Ball of the Eyes, or Hairs of the Brows or Lids, he does not mention. —— Nor is that learned Author ſo exact in ſome other Circumſtances in other Examples of this ſort as could be wiſh'd. However, I think what he ſays of the Duke of *Mantua* deſerves a Remark. —— " Quicquid " ſit, pro vero habendum eſt quod de *Carolo Gonzaga Mantuæ* Duce conſtans fama tulit, levi per " totam cutem facta *frictione* flagrantes ſpecies exire " ſolitas." —— But here alſo it were to be wiſh'd he had let us know whether this great Man, of a moſt illuſtrious Family, had not ſome particular hairy or ſcaly Texture or Covering to his Skin.

By this, I gueſs, you are excited to know how this Author, who liv'd about a hundred Years paſt, ſolves

* *Tho. Bartholinus* de Luce Hominum et Brutorum, *lib.* iii. *Hafniæ* 1669, 8°.

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ſolves theſe Appearances, of which he had profeſſedly wrote. Take it in his own Words.——

"*Ariſtoteles* (l. i. m. m. cx.) docebat —— quod "omnis natura ejus ſit eſſentiæ procreatrix, qualis "ipſa eſt —— enimvero ſunt ad conſervationem ſpe-"ciei omnis, ejuſdem ſingulæ particulæ, vim ſe dif-"fundendi obtinuerunt, et ſpargendi, per individua "multiplicata, ita ne lux primæva et naturalis, ſin-"gulari numinis conſilio, elementorum mixtioni "addita, mole minor intercidat, et extinguatur cum "ſpeciei non revocando caſu, eo modo conſervari "debuit, quo ſerventur omnia, per inſitam naturæ "potentiam ſui generativam, *&c.*"

IV. *A Letter from the Reverend Mr.* John Forſter *to Mr.* Henry Baker *F. R. S. concerning an* Earthquake *at* Taunton.

S I R,

Read June 15. 1748. IN Anſwer to your Inquiries concerning the Earthquake, which happen'd laſt Year on the firſt Day of *July*, when I was at *Taunton* in *Somerſetſhire*, after taking ſome Pains to inform myſelf more particularly what other People obſerved in different Places, you may depend on the Truth of what follows.

Between Ten and Eleven o' Clock at Night, on the ſaid firſt Day of *July* 1747. being myſelf in ſome Company at *Taunton*, we were ſuddenly ſurpriſed with a rumbling Noiſe like diſtant Thunder, which was followed immediately by ſo conſiderable a Motion

Motion of the Earth, that the Chair whereon I ſat rocked under me. The Noiſe and Shaking ſeemed to come from a Diſtance, and approached gradually, in ſuch a manner as if a loaded Waggon had paſſed along; and continued nearly the ſame Time as ſuch a Waggon would require to go about an hundred Yards. The Motion went from South-Eaſt to North-Weſt; which being the Direction of the Street, on one Side whereof the Houſe ſtood, ſome of us imagined at firſt that a Waggon had really gone along*; but, upon running out and enquiring, we found there had been no Waggon: And indeed, as we were ſatisfied afterwards, no Waggon could have been heard or felt in the back Room where we ſat, on account of its too great Diſtance from the Street.

Notwithſtanding this happened between Ten and Eleven o' Clock at Night, when moſt of the Town were in Bed, the Shock was ſo ſenſible, that many People got up very much terrified; and they waking others, the Conſternation ſoon became general; inſomuch that, altho' it was a rainy Night, Numbers of People ran out into their Gardens, and ſpent the Night there, being apprehenſive of other Shocks. The Account then newly brought us of a dreadful Earthquake at *Lima*, being freſh in every body's Mind, contributed to increaſe the Surprize.

A worthy Clergyman, who lives five Miles from *Taunton*, informed me, that the China and Glaſſes upon the Cupboards in his Houſe rattled and ſhook as if they would fall down, and the Bells in his Houſe

* See ſomething like this in *Phil. Tranſ. n.* 455, *p.* 289.

House rang. A Person who was at that time coming on foot to *Taunton* likewise told me, that the Noise seemed to him like the Discharge of Cannon at a Distance, and came rumbling onwards, till the Earth moved under him in such a manner that he could hardly keep upon his Legs: Several others also that were abroad assured me they had much ado to save themselves from falling.

The Extent of this Earthquake, as far as I can learn, was from Sea to Sea; that is, from the *South Chanel* to the *Severn*. It moved from South-East to North-West, and was felt in every Parish through this whole Course, which is in Length about forty Miles: Nor was its Breadth much less; for it was felt at the same time both at *Exeter* and *Crookhorn*, which lie from one another about the same Distance of forty Miles, in a Line directly across its before-mention'd Course.

This, Sir, is the best Account I am able to give; and I shall add nothing more, but that I am

York-Buildings, June 28. 1748.

Your most humble Servant,

John Forster.

P. S. I have heard it reported that there were Flashes of Lightning at the time of the Earthquake; but I neither saw any myself, nor have met with any body that could affirm he did.

V.

V. *A Letter from* John Byrom *M. A. and F. R. S. to the* President, *containing some Remarks on Mr.* Lodwick's Alphabet.

Honoured Sir,

Read June 30. 1748.

HAVING, by your Permission, borrow'd N°. 182. of the *Transactions*, wherein is contained *An Essay towards an Universal Alphabet, by Mr.* Francis Lodwick *F.R.S.* I shall give a brief Account of it; and, in Obedience to your Commands at the last Meeting, endeavour to shew how it may be reduced into less Compass, and set in a plainer Light.

Mr. *Lodwick* premises first the Advantages of such an Alphabet; which I may as well refer to as repeat; they all center in acquiring, describing, or perpetuating the true Sounds of any Language, by a Standard Character for all.

He then defines what a single Sound, what a compounded one, a Vowel, Consonant, Diphthong, and Triphthong is; all which is likewise sufficiently obvious, and needs no Repetition.

To proceed directly, therefore, to his Alphabet, or Collection of all the single Vowels or Consonants which are used in any Language, the Number of Vowels is, according to him, 14, which are, all but 3, expressed in *English* Words in the following Table.

1. *a* — *tall*	8. *ui* — *muis.* Low Dutch
2. *a* — *tallow*	9. *y* — *tile*
3. *a* — *tale*	10. *o* — *tone*
4. *e* — *tell*	11. *u* — *tunne*
5. *ea* — *teal*	12. *u* — *une.* French
6. *i* — *till*	13. *oo* — *tool*
7. *u* — *dure.* French	14. *ou* — *tould.*

These, he ſays, are the Vowels, *each of which is long, and ſhort; ſhort, as in the Words* God, Man, Sin; *long, as in* Ball, Demand, Seen, *&c.*

As any Vowel ſounded by itſelf is naturally long, I take it for granted that he intended the 14 in this Table to be all ſuch; and yet, in the Words *Tallow, tell, till, tunne,* the Vowels, as we now pronounce them at leaſt, are all ſhort; and in the Words *tile* and *tould,* a Diphthong (or Compoſition of 2 Vowels pronounc'd in the Time of one) is ſounded: So that there are but 5 long Vowels accounted for in our Language by the Words *tall, tale, teal, tone, tool.* The foreign Words, as it would be nice and endleſs to diſpute about, ſo it comes not within the Compaſs of my preſent Deſign; which is to give a Liſt of Vowels, whereby to diſcriminate, as conveniently as may be, all the Inſtances of Vocality that occur, diſtinctly, in the *Engliſh* Language; for which I apprehend that half the Number in his Catalogue, or 7 Characters, would be, to all attainable Purpoſes, ſufficient.

As we commonly reckon but 5 Vowels in our Alphabet, *a, e, i, o, u*; two of which, *viz. i* and *u,* are really Diphthongs, I muſt denote the ſingle Vowels

Vowels by 2 apiece, as Cuſtom ſometimes does, to ſhew which I mean; and, beginning from the ſmalleſt Aperture of the Organs to the moſt dilated, they are theſe.

1.	*oo*	*ooze*	*ſtool*	*too*
2.	*oa*	*oat*	*ſtole*	*to*
3.	*ee*	*eel*	*ſteel*	*ſee*
4.	*ea*	*eat*	*ſteal*	*ſea*
5.	*ai*	*aid*	*ſtale*	*ſay*
6.	*aa*	*aaron*	*ſtamen*	(ſol)*fa*
7.	*au*	*autumn*	*ſtall*	*ſaw*

Though vocal Sounds, like inſtrumental, may, in Speculation, admit of numberleſs Diſtinctions, y.t, as Experience ſhews that 7 Notes, flatten'd or ſharpened, upon Occaſion, ſuffice for a practical Gamut, or Scale of Muſic, ſo I incline to think, that 7 vocal Notes or Vowels, varied in ſome correſpondent manner, or ſtruck, as one may ſay, in diphthongal or triphthongal Chords with each other, may well enough account for the Sounds of our Language; or poſſibly of any other, if it be conſider'd, that different Voices, as well as Inſtruments, have ſomewhat ſo peculiar in them, that nothing but the Ear itſelf is able to diſtinguiſh.

However, as far as theſe 7 Vowels extend, if they were denoted by any common Characters, as, ſuppoſe at preſent, by the 7 firſt numeral Figures, the abſurd Variety which Cuſtom has introduc'd of expreſſing the ſame vocal Sounds, amongſt different Nations, even uſing the ſame alphabetical Characters,

ters, would, in a great meaſure, be immediately corrected.

One Inſtance may ſerve for many; the Word *we* in *Engliſh*, and the Word *oui* in *French*, however differently written, have the ſame Sound, or Compoſition of Sound, from our Vowels *oo*, and *ee*, or their *ou* and *i*; if then the Figure 1 was always to denote the Sound that we expreſs by *oo*, the *French* by *ou*, the *Italians* by *u*, &c. and the Figure 3 was to denote our *ee*, and their *i*, in like manner, the combin'd Character, or Diphthong 13, would be ſounded alike by all Readers of any Nation, who ſhould previouſly be agreed upon ſuch a common Character.

And allowing Mr. *Lodwick*'s Notion, that there are 3 Vowels in other Languages, which ours has not, there will remain the Figures 8, 9, 0, to expreſs them by—or any other Characters may be pitch'd upon. What I aim at is to ſhew, that, through an over Purſuit of Accuracy, he has multiplied his Vowels, without any apparent Neceſſity.

And the Caſe is the ſame with reſpect to his Conſonants, which he thus ranges into 11 Files, and 6 Ranks.

	1.	2.	3.	4.		5.
1	B *bond*	D *dark*	J *jeſt*	G *game*		═
2	P *pond*	T *tart*	Ch *cheſt*	K *came*		═
3	M *mind*	N *name*	gn *ſeign* Fr.	ng *ſong*		═
4	═	dh *this*	j *jean* Fr.	g *gaen*	} Low	V *Valley*
5	═	th *thing*	ſh *ſhall*	ch *dach*	} Dutch	F *Folly*
6		n *danſe* French.				

6.

6.	7.	8.	9.	10.	11.
=	L *lane*	H *hand*	Y *yarn*	R *rand*	W *wand*
=					
=					
Z *zeal*	lh Welſh				
S *ſeal*					

By this Diſtribution one is led to think, at firſt, that he would hint that there were in Language 11 times 6, or 66 Conſonants; though Experience had taught him to complete the 6 Ranks of his ſecond File only.

But, as the Mark (=) by which he ſignifies that there are indeed analogous Conſonants that might be expreſs'd in thoſe Places, but with *a Difference that would be too nice for common Diſcernment; and ſuch as he had never heard expreſſed in any Language*; as this Mark, I ſay, occurs but 8 times, I will ſuppoſe him to take the real Number of Conſonants in Nature to be 37, whereof 29 may anſwer the Purpoſes of an univerſal Alphabet.

To reduce both Files and Ranks into leſs Compaſs, and plainer Order, I would take the real Conſonants of his firſt and fifth Files into one File or Rank; and place them in this manner

P. B. M. F. V.

diſmiſſing the 5 unexpreſs'd (and, for any thing that appears, inexpreſſible ones) as imaginary.

And to this Order of Conſonants all ſuch as are uſed in our Language may be adjuſted; for, beginning thus with the labial and labio dental Conſonants, and ſo proceeding to the Gutturals, they will ſtand, in Rank and File, after this manner.

P. B. M. F. V.
S. Z. R. Sh. Zh.
T. D. L. Th. Dh.
K. G. N. Ch. J.

wherein their mutual Analogy and Correſpondence ſeems to appear in the plaineſt Light that one can poſſibly put them in; as a little Attention will better diſcover, than a Prolixity of Particulars explain.

If not, I ſhall be glad to be ſet right, if I am miſtaken in ſuppoſing, 1. That here is no Conſonant omitted, which is really uſed in our common Pronunciation; or, 2. That here are none ſuperfluous, or compounded; or, 3. That, in this View of them, their Relation to each other is the moſt diſcernible.

I except the Letter *H*, which may have its Place amongſt the guttural conſonantal Aſpirates, which ſome foreign Nations are accuſtomed to, but ours, in general, is not: And theſe, as I conceive, will not be found to be diſtinctly more than what the Addition of a fifth Rank to the foregoing may exhauſt.

The Power or Force of this peculiar Letter *h* is ſo capable of Intermixture with that of others in this Table, and that of the Naſo-guttural *N*, of ſliding, without its full Expreſſion, into a following Conſonant (as it does particularly in *French* Pronunciation), that they have led the Writers on this Subject to imagine Conſonants in Nature, which they endeavour to expreſs by *N French*, *gn*, *ng*, and by divers Changes of the Letters *h*, *n*, *g*, that give an attentive Examiner no clear Idea of any diſtinct Conſonant, but rather perplex

perplex the Matter by unintelligible Niceties, and hinder the Prospect of an universal Alphabet, by seeming Impossibilities of arriving at it.

Y and *W*, which constitute Mr. *Lodwick*'s 9th and 11th File of Consonants, are equal to the Vowels *ee* and *oo*: His two *Dutch* Consonants may have their Place in the 5th Rank above-mention'd.

So that omitting such of his 29 as ought, if these Observations are just, to be omitted, there will remain the 20 that are here digested into 5 Files and 4 Ranks, for the real Alphabet of Consonants in the *English* Language; together with the *H*, which whether it may be reckon'd one or not, I shall leave to the Grammarians.

The 5 Files, for an Assistance to the Memory, may by the Help of intermediate Vowels, be compris'd in 5 technical Words of an hexameter Verse, such as,

PaSTiKa, BoZDaGo, MeRLiN, FiShThoChe, VeZhaDhJo.

And the 4 Ranks, as they begin with the Letters *P*, *S*, *T*, *K*, may, for the same Reason, be called *Prima*, *Secunda*, *Tertia*, *Quarta*, (=*Kuarta*) Classis.

These 20 Consonants may appear to be too few; but I much question whether the real Alphabet of any Language has either more in Number, or better fitted for the Purposes of Speech than this of ours: The Difference lies chiefly in the Gutturals, which the *Orientals*, *Welsh*, *Germans*, &c. pronounce differently from us.

Our Neighbours the *French* (which is very remarkable) few as these 20 are, have but 16 of

them; and yet they are able to expreſs themſelves with greater Fluency and Preciſion than we can well be Maſters of, till we ſhall imitate their Care to poliſh and to propagate their Language, by ſome Attention to the Improvement of our own.

They have none of our *th*, *dh*, *ch*, or *j*; and if a Man's Name, ſuppoſe, were *Thatch th' edge*, they would not be able, without previous Practice, to pronounce any one of theſe 4 Conſonants, which help to compoſe it, and which Cuſtom obliges us to denote, ſo abſurdly, by 9 Letters that have not the alphabetical Force of any one of theſe 4 amongſt them.

The moſt important Reflection upon the Subject is this, that whereas we have in our Language but 7 diſtinct Sounds or Vowels, and thrice the Number of Stops or Modifications of them; if we had accordingly 28 Letters or Types appropriated to them, and always wrote or printed what we ſpoke, the Theory of Reading might be acquired in as few Hours, as it coſts at preſent Months or Years to acquire it in.

But I forbear the Purſuit of this Topic any farther; underſtanding, from Gentlemen who were deſirous that I ſhould examine Mr. *Lodwick*'s Scheme immediately, that the *Society* would break up for the Summer, at the next Meeting: Attendance upon my Short-hand Scholars has obliged me to urge what occurr'd upon the Peruſal of it as briefly as I could, and ſo I ſubmit it to Conſideration, and am, *&c.*

VI.

VI. *A* Roman *Inſcription found at* Bath, *communicated to the* Royal Society *by the Rev.* William Stukely *M. D. Fellow of the Coll. of Phyſ. F. R. S. and* Rector *of St.* George *the Martyr*, London.

Read June 30. 1748.

L ▾ VITELLIVS ▾ MAXIMI
NIAI ▾ F ▾ T ▾ ANCINVS ▾
CIVES ▾ HISP ▾ CAVRIESIS
EQ ▾ ALAE VETTONVM ▾ CR
ANN ▾ XXXXVI ▾ STIP ▾ XXVI ▾
H ▾ S ▾ E ▾

Thus to be read. *Lucius Vitellius Maximiniani filius Titus Ancinus,* * *civis Hiſpanus Caurienſis* § *equitum alæ Vettonum curator, annos* XLVI. *ſtipendii* XXVI. *hic ſepultus eſt.*

VII. *Extract of a Letter from the Rev. Dr.* Stephen Hales *F. R. S. to the Rev. Mr.* Weſtly Hall, *concerning ſome* Electrical *Experiments.*

Dear Sir, *Teddington, Feb.* 23, 1746-7.

Read June 30. 1748.

THE Favour of yours of *Jan.* 27. I ſhould have anſwer'd ſooner, but have been prevented by Variety of Buſineſs, eſpe-

 cially

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* Like *nubes*, *labes*, *ſepes*.

§ Of the City of *Coria* in *Spain*.

cially the being much in *London*; where I ſaw laſt Week ſome electrical Experiments; in which new Field of Reſearches there are daily new Diſcoveries made: The active electric Fluid ſeems to be a great Agent, in Conjunction with the Air, in the Production of Fire.

A warm thick Piece of Iron being ſuſpended by two ſilk Lines, had a warm very thick Piece of Braſs laid on it, on which was placed a common Hen's Egg: When electrified, the Flaſhes from the Iron were of a bright ſilver light Colour; from the Braſs (eſpecially near it) the Flaſhes were green; and from the Egg of a yellowiſh Flame Colour; which ſeems to argue, that ſome Particles of thoſe different Bodies were carried off in the Flaſhes, whence theſe different Colours were exhibited.

It is ſuſpected that great Degrees of electrifying have occaſion'd ſome Women to miſcarry; and no Wonder that ſuch ſudden Shocks ſhould do it. I wrote to Mr. *King* the Experimenter to electrify a Frog, while the Circulation of its Blood was viewed with a Microſcope, to ſee if it accelerated its Motion, which he has not yet done.

He obſerves, that a Piece of Linen that has never been waſhed, will ſoon give a good Degree of Electricity to a large warm glaſs Tube; *viz.* on account of the mealy Paſte, which Weavers dreſs the Linen with; and therefore any Piece of Linen thus dreſſed will do.

I gave an Account in the *General Evening Poſt* of *September* laſt of the great Benefit of Ventilators in *Newgate*, and in the *Succeſs* Frigate for *Georgia*, which lay five Months wind-bound in our Chanel with the Tranſports for *Cape Breton*, the reſt of which were all

all very ſickly; but in the *Georgia* Frigate, in which were about 300 Men, all were in good Health; and laſt Week I was informed that they got all in Health to *Georgia*.

* * *

Your obliged and

affectionate humble Servant,

Stephen Hales.

VIII. *Extract of a Letter from* Tho. Aery *M. D. to* Cromwell Mortimer *M. D.* Secret. R. S. *containing the Particulars of the Cure of a Wound in the* Cornea, *and a Laceration of the* Uvea *in the* Eye *of a Woman.*

Whitehaven, June 14. 1748.

S I R,

Read June 30. 1748.

YOUR laudable Endeavours to promote the Art of Medicine, make me willing to hope you will pardon the Freedom I take of acquainting you with the following Caſe, tho' I have not the Happineſs of your Acquaintance; and I beg the Favour of you to lay it before the *Royal Society*, to publiſh, if you approve of it; which will oblige, Sir,

Your humble Servant,

Thomas Aery.

A

A POOR Widow, aged 26 Years, of a pale Complexion, was for feveral Years now-and-then fubject to the Colic. *Dec.* 26. 1744. fhe received a Wound in the *Cornea* of her right Eye, by the Spear of a common Fork, which alfo divided the *Uvea.* Part of the aqueous Humour was difcharg'd, the Eye loft its Tranfparency, had a violent Pain in it, and fhe could only diftinguifh Objects when fhe look'd down. I order'd her a *Collyrium* prepared of the *Balf. Tolut. Camphor. folut. in Sp. Vin. Aq. Plantag. cum pauxillo Tinct. Mart. Mynficht.* A few Drops of this blood-warm was to be ufed frequently; to bleed her largely in the Arm, and her Diet was to confift of Water-Gruel, *Aq. Hord.* and frefh Broth.

Next Day fhe had no Pain in the Eye, but complain'd fhe faw Motes floating before it: I order'd her a Purge of *Infuf. Sennæ*, and an aftringent Fomentation to her Temples and Eyelids. The Day following the Eye was inflamed, and the Lids tumefied, and fhe had a Pain in her Head. The *Collyrium* was changed to Rofe-water and Vinegar, *aa* ℥ fs. Roche-Alum *gr.* v. 3 Drops twice a Day. The 29th the Inflammation increafing, the *Infuf. Sennæ* and Bleeding were repeated, and the Parts were fomented only with Spirit of Wine. The 31ft the Inflammation continued to decreafe, till after a Fright, *Jan.* 5. the Inflammation increafing, the Sides of the Wound became a little protuberant. The *Senna* was repeated, and a Blifter laid behind the right Ear, and an emollient *Collyrium* was ufed:

Next

4

Next Day the Swelling of the Eyelids was gone: The 11th she had a Shew of the *Menses*, and the Wound appeared healed: From the 15th to the 24th the Inflammation continued to abate; only one Day it increased by fretting and weeping much; but by bleeding she grew better, and so she continued to the 30th; unless one Day, upon catching Cold, her Eye became exceedingly inflamed, which was relieved by bleeding. *Feb.* 4. she had a little Pain in her Eye, and the *Tunica adnata* looked a little red. Soon after dropping in of 2 Drops of cold Water, the Eyelids swelled, and a violent Inflammation of the Eye ensued, with a Speck appearing; but these Symptoms went off by repeated Applications of Leeches and a mercurial Purge. The 19th a Sternutatory of Hellebore and *Euphorbium* was order'd.

In a few Days after the Inflammation left her Eye; when she complained she saw double; which Complaints also soon left her.

The Eye is myopical, and she sees the right Side of Objects a little darkened; yet she can read pretty small Characters. The *Uvea* is not united where it was divided, but still retains its natural Power of Contraction; the Transparency of the Humours and Convexity of the *Cornea* are the same as before; there is no Scar upon the *Cornea*; the Shape of the *Pupil* is much alter'd, as may be seen by the Figure of the Eye, which I send herewith. See *Fig.* p. 415.

Upon catching Cold she is subject to a slight Pain in her Eye. At present there remains no other Alteration than what I have just mention'd, and what necessarily follows from the Contraction of the *Pupil*, the not admitting a sufficient Quantity of Rays

to

to paſs to the *Retina*, upon which Account ſhe is ſhort-ſighted. Her ſeeing Objects darkened on one Side, may proceed from the artificial Part of the *Pupil* being ſituate nigher to the great *Canthus* of the Eye than uſual in Nature; by which the Rays which fall on the Side of the *Cornea* next to the little *Canthus* of the Eye, being partly intercepted, muſt occaſion a Defect in the Pictu e; from which Defect a Darkneſs will be ſeen on one Side of the Object. To the Weakneſs of the Veſſels of the Eye we may attribute the Pain of the Eye upon catching Cold: It oft-times happens to thoſe who have had a ſevere *Ophthalmia*, that, during Life, the ſmall Veſſels are too weak; and hence, from ſlight Cauſes being diſtended, they will be painful, and frequently red.

This Caſe is known to ſeveral in this Place, particularly to Mr. *Blencowe*, an ingenious Surgeon and Apothecary.

Some Remarks occur from this Caſe; *viz.*

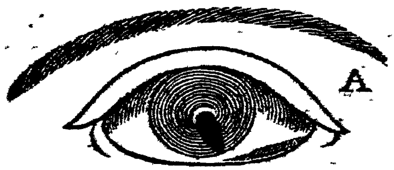
1. Her Cure would have been performed ſooner, if her Circumſtances had allowed of her obſerving an exact Regimen.
2. When her Eye had little or no Appearance of Inflammation, I tried cold Water, but with rather bad Succeſs. All cold Applications to inflamed Eyes, Aſtringents or Repellents, require the utmoſt Caution in applying them; for if they produce not a good, they will produce a bad Effect. In ſlight Caſes they oft have very happy Effects, but where the obſtruent Matter is ſo fixt that it will not ſuffer itſelf to be eaſily repell'd back, the, Veſſels being ſtraitened, the Fluids coagulated the

the Diſeaſe will be increaſed; which happen'd in this Caſe from the Application of cold Water.

3. The good Effects of Evacuations are very evident in abating the Inflammation. Wounds in the *Cornea*, attended with a Wound of the *Uvea*, and a troubleſome Ophthalmy, heal without any Scar.

5. An artificial *Pupil*, made by ſlitting the *Uvea* in a different Method from that invented by the ingenious Mr. *Cheſelden*.

6. The Neceſſity of changing the Topics, according to the State of the Diſtemper; which has been remarked before by ſeveral celebrated Authors.



A, This Figure repreſents the Eye as it appears at preſent.

The SOCIETY *adjourned to* October. 27. 1748.

IX. *Tables of* specific Gravities, *extracted from various Authors, with some observations upon the same; communicated in a Letter to* Martin Folkes *Esq;* President *of the* Royal Society, *by* Richard Davies *M.D.*

Presented Feb. 18. 1747.

THE manifold applications which may be made, for the purposes of Natural Philosophy, of the relations which Bodies bear to each other, by their respective Specific Gravities, engaged me some years since to collect all the experiments of this sort I could meet with in the course of my studies, and also to make several new ones of my own with the same design.

When my collection began to be somewhat considerable, I disposed the several bodies in Tables according to their species, which I found to be the most convenient method, as my tables were by this means capable of receiving additions in any part, without destroying the form of the whole: and as they were thereby easy and ready to be consulted, and well disposed for the forming of immediate comparisons between the several bodies of the same species.

But having now no farther opportunities of enlarging my collection, I hereby beg leave to recommend the prosecution of my design to others, as a subject well deserving the attention of some of the members of the *Royal Society*, to whom I therefore present these my tables; wishing they may prove of

some

ſome uſe and ſervice to the inquiſitive and philoſophical part of the world. As I perſuade myſelf they really will, when they ſhall be further rectified, by the omiſſion of the erroneous or uncertain experiments; when they ſhall be enlarged by the addition of ſuch others, as may ſtill be found in good authors, or which yet remain unpubliſhed in the cloſets of the curious: and eſpecially if ſome ſuch gentlemen as have skill, leiſure, and opportunities, ſhall pleaſe to ſupply their remaining defects, by the communication of their own obſervations, made upon thoſe bodies, whoſe ſpecific gravities have not as yet been carefully recorded.

Denique cur alias aliis præſtare videmus
Pondere res rebus, nihilo majore figura?
Nam, ſi tantundem eſt in Lanæ glomere, quantum
Corporis in Plumbo 'ſt, tantundum pendere par eſt.
Lucret.

A short account of the Authors, from whose writings and experiments, the following Tables have been collected, with some remarks upon the experiments themselves, and the manner in which they appear to have been made.

THE antients have left but few particulars concerning the different specific gravities of bodies, tho' it is plain they were in the general sufficiently acquainted with them. It was by the knowledge of the various weights of gold and silver, that *Archimedes* is recorded to have detected the famous fraud committed in *Hiero's* crown, as *Vitruvius* has at large related in his *Architecture*, l. ix. c. 13. and it is from the same great philosopher, that we have derived the demonstration of those hydrostatical rules, by which the proportions are best to be known, of the several weights or densities of different bodies, having the same bulk or magnitude: as may be seen in his tract *De insidentibus humido*, lost in the *Greek* original, but retrieved in great measure, as it is said, from an *Arabic* translation. It was published in *Latin*, with a commentary by *Federicus Commandinus* at *Bononia* 1565, 4°, and the substance of it by Dr. *Barrow* in his *Archimedes*, printed likewise in 4° at *London* 1675.

Pliny, in the xviii. book of his *Natural History*, has set down the proportional weights of some sorts of grain, among which he says that barley is the lightest. *Levissimum ex his hordeum, raro excedit, [in singulos nimirum modios]* xv *libras, et faba* xxii. *Ponderosius*

Ponderosius far, magisque etiamnum triticum. And a little further on, *ex his generibus* [*frumenti scilicet*] *quæ Romam invehuntur, levissimum est Gallicum, atque e Chersoneso advectum: quippe non excedunt in modium vicenas libras, si quis granum ipsum ponderet. Adjicit Sardum selibras, Alexandrinum et trientes: hoc et Siculi pondus. Bœoticum totam libram addit: Africum et dodrantes. In Transpadanâ Italiâ scio vicenas quinas libras farris modios pendere: circa Clusium et senas.* And the same author in his xxxiii. book, speaking of quicksilver, observes that it is the heaviest of all substances, gold only excepted. *Omnia ei innatant, præter aurum: id unum ad se trahit.* Which *Vitruvius* had also taken notice of, and had mentioned besides the weight of a known measure of it, that of four *Roman Sextarii. Eæ autem* [*guttæ nempe argenti vivi quæ inter se congruunt et una confunduntur*] *cum sint quatuor sextariorum mensuræ, cum expenduntur, inveniuntur esse pondo centum. Cum in aliquo vase est confusum, si supra id lapidis centenarii pondus imponitur, natat in summo: neque eum liquorem potest onere suo premere, nec elidere, nec dissipare: centenario sublato, si ibi auri scrupulum imponatur, non natabit, sed ad imum per se deprimetur, Ita non amplitudine ponderis, sed genere singularum rerum gravitatem esse, non est negandum. Archit.* l. vii. c. 8.

Again, *Q. Rhemnius Fannius Palæmon,* in his fragment *De ponderibus et mensuris,* has given us an observation, of the proportional gravities of Water, Oil, and Honey.

——Libræ

——*Libræ, ut memorant, bessem sextarius addet,*
Seu puros pendas latices, seu dona Lyæi,
Addunt semissem Libræ labentis Olivi,
Selibramque ferunt mellis superesse bilibri.

That is to say, that the *Sextarius* of either water or wine weighed 20 ounces, the same measure of oil 18, and of honey 30. Their specific weights were therefore in proportion as 1.0, 0.9, and 1.5, exactly agreeable to what *Villalpandus* determined about the beginning of the last century: Yet was this author himself sensible that these were not to be look'd upon as very nice experiments.

Hæc tamen assensu facili sunt credita nobis.
Namque nec errantes undis labentibus amnes,
Nec mersi puteis latices, aut fonte perenni
Manantes, par pondus habent: non denique vina,
Quæ campi aut colles nuperve aut ante tulere,
Quod tibi mechanica promptum est depromere; Musa.

After which he proceeds to describe a good pretty instrument for the ready finding of the different specific gravities of fluids, and shews how those of solids also may be hydrostatically discovered. And so much shall suffice for what I had to mention from the antients relating to this subject: I now come to those who have written within these last hundred and fifty years.

Francis

Francis Bacon, Lord Verulam &c. in his *Historia densi et rari*, printed in the second volume of his works in *folio*, *London* 1741. *p*. 69. has given a table, which he calls, *Tabula coitionis et expansionis materiæ per spatia in tangibilibus (quæ scilicet dotantur pondere) cum supputatione rationum in corporibus diversis*. This tract does not appear to have been published till after his death, which happened in the year 1626, but was probably written several years before; and the experiments were even as he tells us made long before that. *Hanc Tabulam multis abhinc annis confeci, atque ut memini, bona usus diligentia.* I therefore apprehend it to be the oldest table of Specific Gravities now extant. The experiments therein mentioned were not made hydrostatically, but with a cube of an ounce weight of pure Gold, as he says, to which he caused cubes of other materials to be made equal in size: as he did also two hollow ones of silver, and of equal weights, the one to be weighed empty, and the other filled with such liquid as he wanted to examine. He was himself sensible that his experiments of this sort were, notwithstanding his care, very defective, *possit proculdubio tabula multo exactior componi, videlicet tum ex pluribus, tum ex ampliore mensura: id quod ad exactas rationes plurimum facit, et omnino paranda est, cum res sit ex fundamentalibus*. From among these, notwithstanding their imperfection, as they appear to have been some of the first experiments of the sort regularly digested, and as they were besides made by so great a man, I have extracted the specific gravities of the fixed metals, which I have inserted as examples in the following tables: after reducing them to the common

form, upon the ſuppoſition that pure gold was, according to *Ghetaldus*, juſt 19 times as heavy as water. And this I have rather choſen to do, than to make uſe of his Lordſhip's own weight of water given in the table, which in the manner he took it could not be very exact, and which beſides would not have brought out the ſpecific gravity of pure gold more than 18 times as much; and that of the other metals in proportion. This table contains in all 78 articles.

There are alſo in the third volume of the ſame edition of his works, *p.* 223, *Certain experiments made by the Lord Bacon about weight in air and water*. Theſe are truly *hydroſtatical*, but very imperfect, I have not therefore inſerted any of them in the following collection.

Marinus Ghetaldus, a nobleman of *Raguſa*, publiſhed in *quarto* at *Rome*, in 1603, his treatiſe entitled, *Promotus Archimedes, ſeu de variis corporum generibus gravitate et magnitudine comparatis*, wherein he has given a compariſon between the ſpecific gravities of water and eleven other different ſubſtances, from his own hydroſtatical experiments made with care and exactneſs. Theſe I have inſerted: expreſſing the numbers as they ſtand in his own book, but I have afterwards alſo for uniformity reduced them to the decimal form. I have beſides at the end tranſcribed at large the two tables of this author, in which every one of the twelve ſorts of bodies he treats about is ſucceſſively compared with all the others, both in weight and magnitude.

Father

Father *Johannes Baptiſta Villalpandus*, a Jeſuit of *Cordoua* in *Spain*, in his *Apparatus Urbis et Templi Hieroſolymitani*, printed in *folio* at *Rome* in 1604, exhibited a table of the proportional weights of the ſeven metals and ſome other ſubſtances, from his own experiments, made with great care as he tells us, by the means of ſix equal ſolid cubes of the fixed metals, and a hollow cubical veſſel 8 times as large, for the comparing Mercury, Honey, Water, and Oil with the ſame. His numbers, which are inſerted under his name in the following tables, were alſo again publiſhed afterwards by *Joh. Henr. Alſtedius* in his *Encyclopædia univerſa*, printed in 2 vols. in *folio* at *Herborn* 1630, and by *Henry Van Etten*, in his *Mathematical recreations*, from whence they have been often tranſcribed into other books. *Villalpandus*'s book, which is only the third volume of a work begun to be publiſhed ſeveral years before, was itſelf printed ſo ſoon after *Ghetaldus*'s, that it is probable he either never ſaw that author, or not at leaſt till after his own experiments were made.

Mr. *Edmund Gunter*, in his *Deſcription and Uſe of the Sector*, printed after his death by Mr. *Samuel Foſter* in 1626, having occaſion to make mention of the ſpecific weights of the ſeveral fixed metals, quoted *Ghetaldus*, and made uſe of his proportions, and ſo did alſo Mr. *William Oughtred*, in his *Circles of Proportion*, firſt publiſhed in *quarto* 1633, with this only difference, as to the form, that he changed *Ghetaldus*'s unit into 210, whereby he expreſſed all his relations in whole numbers. It is likewiſe probable that *D. Henrion* took from the

 ſame

ſame place the numbers he applied in his *Uſage du Compas de Proportion*, printed at *Paris* in 1631, 8°. although he has not given them all with exactneſs, for the ſake as it ſeems of uſing ſimpler vulgar fractions.

Father *Marinus Merſennus*, a *French* Minim, in his *Cogitata Phyſico-Mathematica*, printed at *Paris* in 1644. 4°, has given from the obſervations of his accurate friend *Petrus Petitus*, a table of the ſpecific gravities of the metals and ſome other bodies, making Gold 100, Water $5\frac{1}{3}$, and the reſt in proportion. Theſe I have reduced to the common form, and inſerted under his name in the following tables. The ſame were afterwards made uſe of by Father *Francis Milliet de Chales*, Jeſuit, in his *Curſus Mathematicus*, Monſieur *Ozanam*, Profeſſor *Wolfius*, and ſeveral others. I have not ſeen *Petitus*'s own book, but it was entitled *L' Uſage ou le moyen de pratiquer par une Regle toutes les Operations du Compas de Proportion—augmentées des Tables de la Peſanteur et Grandeur des Metaux* &c. had a privilege dated in 1625. tho' it is ſaid not to have been printed till ſome years after. The ſame Father *Merſennus* has alſo taken notice, in his general preface, of a table of 20 ſpecific gravities, ſome time before publiſhed by Monſ. *Aleaume*, which he there ſets down, but which he alſo obſerves to be very incorrect. I have not therefore inſerted any of them in this collection.

Mr. *Smethwick*, one of the earlieſt members of the *Royal Society*, communicated to the ſame in *July* 1670, the weights of a cubic inch of ſeveral different ſubſtances;

ſubſtances; ſaid to have been formerly taken by Mr. *Reynolds* in the *Tower* of *London*. This gentleman was the ſame who compoſed ſeveral tables relating to the price of Gold and Silver, which were publiſhed in a book entitled *The Secrets of the Goldſmith's Art*, at *London* 1676, *in octavo*. Theſe weights are expreſſed in decimals of an *Averdupois* Pound, are carried to 8 places of figures, and ſeem to have been carefully and accurately collected. I have therefore in the following tables reduced them to the common form, in order to give them their proper authority with the reſt. I am ignorant whether theſe weights were ever before printed or not, neither can I give any account, after what particular manner the experiments were made, from which they were taken. They were communicated to me from the regiſter-books of the *Royal Society*; and I ſhall only obſerve, that the abſolute weight here aſſigned of a cubic inch of common water does not differ more than a ſmall fraction of a grain, from the weight of the ſame afterwards determined by Mr. *Ward* of *Cheſter*.

The *Philoſophical Society*, meeting at *Oxford*, directed ſeveral experiments to be made hydroſtatically by their members, concerning the ſpecific gravities of various bodies; which being digeſted into a table, were by Dr. *Muſgrave* communicated to the *Royal Society* the 21st day of *March* 1684. ſoon after which they were printed in the 169th number of the *Philoſophical Tranſactions*. Theſe experiments were, according to Dr. *Muſgrave*, made by Mr. *Caſwell* and Mr. *Walker*: they are all originals,

and eſteemed ſome of the moſt accurate that are extant.

The honourable *Robert Boyle*, at the end of his *Medicina hydroſtatica*, firſt publiſhed at *London* in 1690, 8°. ſubjoined a table of the ſpecific gravities of ſeveral bodies, accurately taken from his own hydroſtatical experiments. Beſides which, there are alſo in the ſame tract, and in other parts of his works, ſeveral experiments of this excellent author's, which he has given occaſionally, together with the uſes reſulting from them. To ſuch of theſe in the following collection, as were taken from the table juſt mentioned, I have barely annexed his name, but to ſuch of the others as occurred, I have alſo added the volume, page, and column, of the late *folio* edition of his works in 1744, where the ſame are to be found. It may be noted, that in the firſt edition of the *Medicina hydroſtatica*, there were ſeveral errors of the preſs. Such of them as I could diſcover by calculation, I have corrected in the following pages.

There is a table publiſhed under the name of *J. C.* in the 199th number of the *Philoſophical Tranſactions*, A°. 1693: and this is evidently a ſupplement to that above-mentioned of the *Philoſophical Society* meeting at *Oxford*. The experiments were, according to the initials *J. C.* made by the ſame curious perſon Mr. *John Caſwell*, and are therefore of the ſame eſtimation as the others.

Mr. *Homberg*, of the *Royal Academy of Sciences* at *Paris*, read a memoir in 1699, wherein he took no

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tice of the expanſion of all ſubſtances by heat, and the contraction of the ſame by cold: from whence it muſt follow, that the ſpecific gravities of the ſame bodies would conſtantly be found leſs in the ſummer and greater in the winter. And this he ſhew'd from the experiments he had made upon ſeveral fluids, both in the ſummer and the winter-ſeaſons, by means of an inſtrument he had contrived and called an *Aræometer*, being a large phial, to which he had adjuſted a long and ſlender ſtem, whereby he could to good exactneſs determine, when it was filled with equal bulks or quantities of the ſeveral fluids he propoſed to examine. The reſult of his trials with this inſtrument he digeſted into a ſhort table, which was printed in the memoirs of the *Academy* for the ſame year 1699. This table *John Caſpar Eiſenſchmid* afterwards republiſhed with ſeveral additions, in his tract *De Ponderibus et Menſuris*, printed at *Strasburg* in 1708, 8°. changing it to a more convenient form for his purpoſe, by reducing the different fluids therein named to the known bulk of a cubical *Paris* inch. So much of this table as I thought might be of ſervice, I have here ſubjoined to the others in the following collection, but I have alſo made an alteration in the form, the better to fit it for general uſe, by omitting the abſolute weights of the ſeveral bodies in ſummer and winter, and placing inſtead of them, after the name of each body a decimal number, expreſſing the proportion of its weight in winter to its weight in ſummer, ſuppoſed to be every-where repreſented by unity.

Sir

Sir *Iſaac Newton* Kn^t. in his *Opticks* printed in 4°. at *London* 1704, gave a table of the ſpecific gravities of ſeveral *diaphanous* bodies. The experiments were made by him with a view chiefly to optical enquiries, and to enable him to compare their denſities with their ſeveral refractive powers: we may therefore be well aſſured that they were made by the great author with the moſt ſcrupulous care and exactneſs. The table conſiſts of 22 articles.

John Harris D.D. in his *Lexicon Technicum*, firſt printed at *London* in 1704, *fol.* republiſhed at large the ſeveral tables of ſpecific gravities of the *Oxford Society* and *I. C.* from the *Philoſophical Tranſactions*, and that of the honourable *Robert Boyle* from his *Medicina hydroſtatica*, to which laſt he alſo added ſome experiments of his own, made as it ſeems with good accuracy. Theſe are here extracted, and placed under his name in the following tables.

Mr. *John Ward* of *Cheſter*, in his *Young Mathematician's Guide*, firſt printed, as I take it in 1706, acquaints us, that he had himſelf for his own ſatiſfaction, made ſeveral experiments upon the different ſpecific gravities of various bodies; and that he was of opinion, that he had obtained the proportion of the weight that one body bears to another of the ſame bulk and magnitude, as nicely as the nature of ſuch matter, as might be contracted or brought into a leſſer body (*viz.* either by drying, hammering, or otherwiſe) would admit of. And he has accordingly

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given us in the ſaid book the weight of a cubic inch of 24 different ſubſtances, both in *Troy* and *Averdupois* ounces and decimal parts of an ounce; which he further aſſures us requir'd more charge, care, and trouble, to find out nicely, than he was at firſt aware of. This table appears to have been well-eſteem'd, and to have had the ſanction of Mr. *Cotes*'s approbation, by his taking it, when reduced to the common form, into that collection which he drew up for his own hydroſtatical lectures.

Roger Cotes M A. and *Plumian Profeſſor of Aſtronomy and experimental Philoſophy* at *Cambridge*, firſt giving about the year 1707 a *Courſe of Hydroſtatical and Pneumatical Experiments*, in conjunction with Mr. *Whiſton* in that Univerſity, drew up, for the uſe of that courſe, a very accurate Table of Specific Gravities, collecting from ſeveral places ſuch experiments as he took to be moſt exact, and the beſt to be depended upon. And as the judgment of ſo great a man cannot but give a general reputation to ſuch experiments as he had ſo ſelected, I have thought proper, in the following tables, to diſtinguiſh all ſuch by the addition of the letter *C*, after the names of ſuch perſons from whom they firſt appear to have been taken, adding alſo the name of *Cotes* at length, to ſuch others as I have not met with elſewhere, and which I therefore take to have been tranſcribed from the *memoranda* of his own experiments. This table of Mr. *Cotes*'s uſed firſt to be given in *M.S.* to thoſe who attended his lectures; but it was afterwards printed in a ſingle ſheet, relating to a *Courſe of Experiments* at *Cambridge* in

in 1720, and ſince in Mr. *Cotes's Hydroſtatical and Pneumatical Lectures*, when they were publiſhed at large in 8°. by his ſucceſſor Dr. *Smith*, now the worthy Maſter of *Trinity College*. In theſe printed Lectures were inſerted the gravities of Human Blood, its *Serum*, &c. from Dr. *Jurin*, inſtead of thoſe that had before been made uſe of from Mr. *Boyle*.

Mr. *Francis Hauksbee*, now Clerk to the *Royal Society*, did, about the year 1710, begin, in conjunction with Mr. *Whiſton*, who had then newly left the Univerſity, to give hydroſtatical lectures *&c.* in *London*; for the purpoſe of which he reprinted in a thin volume in 4°, in which are the ſchemes of his experiments, Mr. *Cotes*'s table of Specific Gravities above-mentioned. To which he added, from tryals of his own, the weights of Steel, ſoft, hard, and temper'd, which are printed with his name in the following Tables, as are alſo ſome other experiments, which he has ſince occaſionally made, and communicated to me. Mr. *Cotes*'s table, with the above-mention'd additions of Mr. *Hauksbee*, was afterwards again publiſhed by Dr. *Shaw*, in his *Abridgment* of Mr. *Boyle*'s *Philoſophical Works*, at *London*, 1725, 4°. vol. ii. p. 345.

John Freind M. D. at the end of his *Prælectiones Chymicæ*, printed at *London* in 1709, 8°. has publiſhed ſome new tables of the Specific Gravities both of ſolid and fluid bodies, entirely taken from his own original experiments. And as theſe tables contain an account of a very uſeful ſet of bodies, upon which few or no other experiments have been made: it is great

pity

pity that this truly learned and elegant writer was not more accurate in his tryals than he appears to have been. Many of his experiments having indeed been made in ſo lax and improper a manner, and ſo many errors having been committed in them, that one can not with ſecurity depend upon theſe tables, tho' containing otherwiſe facts one would ſo much deſire to be truly informed about. I have however here inſerted the ſeveral particulars of his two laſt tables, which immediately concern Specific Gravities, after correcting ſuch errors in calculation as I could certainly come at: And I hope that I ſhall be excuſed for this free cenſure upon part of the works of a gentleman, who has ſo well deſerved of the learned world, and acquired ſo juſt a reputation in it.

James Jurin, M. D. and ſeveral years Secretary of the *Royal Society*, gave, in N°. 361 of the *Philoſophical Tranſactions*, A°. 1719, ſome original and very accurate experiments made by himſelf, upon the Specific Gravity of Human Blood, at ſeveral times during the ſix preceding years. Theſe were accompanied with a very curious diſcourſe, which has ſince been tranſlated by himſelf, into *Latin*, and reprinted in his *Diſſertationes Phyſico Mathematicæ*, *Lond.* 1732. 8°.

This gentleman has alſo, in N°. 369 of the ſame *Tranſactions*, obliged us with ſome very judicious and uſeful remarks, relating to the *caution to be uſed in examining the ſpecific gravity of ſolids, by weighing them in water*; for want of attending to which, ſeveral ſorts of bodies, ſuch as human *Cal-*

 culi,

culi, the ſubſtance of all woods, *&c.* have appeared, from their pores and ſmall cavities filled up with air, to be conſiderably lighter than they really are.

John Woodward M. D. and Profeſſor of Phyſic in *Greſham College*, had, as he acquaints us in ſeveral places of his works, made a great number of experiments upon the ſpecific weights, of mineral and other foſſil bodies, but which being probably contained in thoſe of his papers which he ordered to be ſuppreſſed at his death, are thereby loſt to the world, to which they would without all doubt have been very acceptable. All I have been able to pick up are a very few mentioned in the *Catalogue of the Engliſh Foſſils in his Collection*, publiſhed ſince his deceaſe, in 8°. at *London* 1729.

Mr. *Gabriel Fahrenheit* F. R. S. communicated, in N°. 383. of the *Philoſophical Tranſactions*, *A Table of the Specific Gravities of* 28 *ſeveral ſubſtances*, from hydroſtatical experiments of his own, made with great care and exactneſs; to which he ſubjoined ſome obſervations upon the manner in which his trials were performed, together with a deſcription of the inſtruments in particular which he made uſe of to examine the gravities of Fluids. To ſome of his experiments which he thought required a greater nicety, he has affixed an aſteriſk in his table, ſignifying ſuch to have been adjuſted to the temperature of the air, when his Thermometers ſtood at the height of 48 degrees. This gentleman, who is well known by the reputation of his Mercurial Thermometers, which he made with great

great curiosity, and which are now generally used, was in *England* in the year 1724.

Professor *Peter van Muschenbroek*, of *Utreckt*, published in his *Elementa Physicæ* at *Leyden* in 8°. 1734. a large table of Specific Gravities, which he afterwards yet somewhat further enlarged in his *Essai de Physique* in *French*, at *Leyden* 1739. 4°. This table contains almost all the preceding ones, but without the names of the authors from whom they were collected. I have among those which follow inserted, under this author's name, such experiments as I had not before met with elsewhere: making use of the *Latin* edition as the more correct, except in such articles which are only to be found in the *French*.

Mr. *John Ellicott* F. R. S. having an opportunity in the year 1745. to examine the weight of some large Diamonds, he accordingly, with the utmost care, and with exquisite assay-scales which very sensibly turned with the 200th part of a grain, took the specific gravities of 14 of those Diamonds, 4 of which came from the *Brasils*, and the other 10 from the *East Indies*. These experiments he communicated to the President of the Royal Society, who caused them to be read at one of their meetings, and afterwards published them in N°. 476. of the *Philosophical Transactions*. Among these *Brasilian* Diamonds, one was of the absolute weight of 92,425, another of 88,21; and among the *East-Indian* ones, one of 29,525 *Troy* grains. And as the size of these stones made them much

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fitter for these enquiries, than any others which had probably ever before been used for the same purpose, so the known accuracy of the author, the goodness of his instruments, and the consistency of all his experiments, sufficiently shew the specific gravities he has delivered in his paper, may entirely be depended upon.

The same curious person also communicated the Specific Gravities of fine and standard Gold, published under his name in the following tables, and which were deduced from experiments he was so kind as to make on purpose at my request.

As I have just had occasion to mention Diamonds, it may possibly not be foreign to the purpose here to take some notice of the Diamond Carat weight, used among jewellers, which weight was originally the Carat or 144th part of the *Venetian* ounce, equal to 3,2 *Troy* Grains, but which is now, for want of an acknowledged standard, somewhat degenerated from its first weight. I have myself found it, upon a medium of several experiments, equal to 3,17 *Troy* Grains; and I have the rather taken notice of this weight here, because there happens to be a mistake about it, both in Dr. *Arbuthnot*'s and Mr. *Dodson*'s tables, who have set down as it seems the number of Diamond Carats in a *Troy* Ounce, instead of the weight of the Diamond Carat itself. This Carat is again divided into four of its own Grains, and those into halves and quarters, commonly called the eighths and sixteenths of a Carat: and thus the largest of the Diamonds just above-mentioned, weighed, in the jewellers phrase, better than 29 Carats and almost half a Grain.

Mr *James Dodson*, in his book called *The Calculator*, printed in 8°. at *London* in 1747, has inserted a useful table of Specific Gravities, in which he has by the first initial letter of their names distinguished the several authors he has quoted: and amongst these are several new experiments marked with an *L*, which I am told were communicated from his own trials, by Mr. *Charles Labelye*, engineer, and which concern particularly the weights of several sorts of stone and other materials used in building. These I have also distinguished by an *L*. as they stand in Mr. *Dodson*'s book.

Mr. *Geo. Graham*, F.R.S. made for me, at the request of a friend, some accurate trials upon the weight, of Gold and Silver, both when reported fine, and when reduced to the *English* Standard: all which I have inserted under his name in the following tables. Wherein I have besides reported some other single Experiments which I occasionally met with, from *Frederick Slare* M.D. *John Keill* of *Oxford*, M.D. *Stephen Hales* D.D. and *Edward Bayley* of *Havant* in *Hampshire*, M.D.

Rickard Davies M.D. I have lastly to this Collection of Experiments added some of my own, which I endeavoured to make with as much accuracy, as the instruments I was provided with would allow of. My hydrostatical Balance was one constructed several years since by Mr. *Francis Hauksbee*, which I have constantly found to turn sensibly with half a grain: and the bodies upon which I made most of my trials, were taken from a collection of the *Materia Medica* formerly made by Signor

Signor *Vigani*, and still preserved in the library of *Queen's College* in *Cambridge*.

TABLE I.

Of Metals.

☉ GOLD, fine. *Ward, C.*	19.640
A Medal esteemed to be near fine Gold *J. C.*	19.636
Or d'essai, ou de Coupelle. *Muschenbr.*	19.238
Fine Gold hammer'd. *Ellicot.* . . .	19.207
D°. an ingot so accounted, and again refined with Antimony. *Ellicot.* .	19.184
D°. the ingot itself just mention'd. *Ellicot.*	19.161
A Medal of the Royal Society, reported fine Gold. *Graham.* . .	19.158
A gold medal of Qu. Eliz. *J. C.* . .	19.125
D°. of Qu. Mary. *J. C.* . .	19,100
Aurum. *Fahrenheit.*	19.081
Id. *Ghetaldus.* Aurum purum. *Bacon* (ex hyp.)	19.000
A gold Coin of Alexander's. *J. C.* .	18.893
Gold. *Reynolds.* . . .	18.806
Aurum. *Villalpandus. Petitus.* .	18.750
Standard Gold (by which is understood Gold of 22 Carats, or such of which our Guineas are intended to be coined). *J. C. Ward. C.*	18.888
An old Jacobus. I suppose the scepter'd broad piece. *Harris.* . .	18.375
A Mentz gold Ducat. *J. C.*	18.261

Aureus

Aureus Ludovicus. *Muſschenbr.* .	18.166
A five Guinea piece of K. James II. 1687. with an Elephant. *Graham.* . . .	17.933
A Portugal piece of 3l. 12s. 1731. ſuppoſed to be nearly the ſame as Standard. *Graham.* . . .	17.854
Guineas, ten weighed together. *Davies.*	17.800
D°. on a mean of 7 trials upon thoſe of different reigns. *Ellicot.*	17.726
A piece of Gold Coin of the Commonwealth. *Harris.* . . .	17.625
Guineas two new ones. *Hauksbee.* .	17.414
A Grain of Scotch Gold, ſuch as Nature had made it. *Boyle* V. 30. *b.* . . $12\frac{2}{7}$	12.286
Electrum, a British Coin. *J. C.* . . .	12.071
QUICKSILVER. Mercurius crudus. *Freind.*	14.117
Mercury Spaniſh. *Boyle* V. 10. *b.* Mercure ſublimé 511 fois. *Muſschenb.*	14.110
Quickſilver. *Oxford Soc.*	14.019
D°. *Ward.* *C.* revived from the Ore. *Boyle.*	14.000
Fine Mercury. *L.*	13.943
Quickſilver, another Parcel. *Oxf. Soc.*	13.593
Mercure amalgamé avec de l'Argent, affiné et ſublimé 100 fois. *Muſschenb.*	13.580
Mercurius. *Fahrenheit.*	13.575*
Argentum vivum. *Ghetaldus.* $13\frac{4}{7}$.	13.571
Mercure amalgamé avec de l'Or affiné, et ſublimé 100 fois; le même meſlé avec du Plomb, enſuite converti en poudre et revivifié. *Muſsch.*	13.550

Coarſe

Coarse Mercury. *L.*	13.512
Mercurius. *Petitus.*	13.406
Quickfilver. *Reynolds.*	13.147
♄ LEAD. *Reynolds.*	11.856
Plumbum. *Villalpand.*	11.650
Id. *Ghetaldus* $11\frac{1}{2}$.	11.500
Id. *Bacon.*	11.459
Lead. *Harris.*	11.420
Hardeft Lead. *L.*	11.356
Plumbum. *Fahrenheit.*	11.350
Lead. *Oxford Soc. Ward.*	11.345
Plumbum. *Petitus.*	11 343
Lead. *Harris.* (an ordinary Piece)	11.330
D°. *Cotes.*	11 325
Plumbum Germanicum. *Mufchenb*	11.310
Caft Lead. *L.*	11.260
☾ SILVER, fine. *Ward. C.*	11.091
A Medal of the Royal Society, reported fine Silver. *Graham.*	10 484
Argentum. *Fahrenheit.*	10.481
Silver. *Reynolds.*	10.432
Argentum. *Villalpandus.*	10.400
Id. *Ghetaldus.* $10\frac{1}{3}$.	10.333
Id. *Bacon.*	10.331
Id. *Petitus.*	10.219
Sterling or Standard Silver (that is, Silver 11 oz. 2dwt. in the pound fine) A half crown of K. William's Coin. *Harris.*	10.750
D°. ftruck into money. *L.*	10.629
D°. *J. C. Ward. C.*	10.535
D°. Caft. *L.*	10.520

A

A new Crown-piece. 1746. LIMA under the head. *Graham.*	10.284
COPPER. *Reynolds.* . .	9.127
Cuprum. *Villalpandus.* . .	9.100
Æs. *Ghetaldus.* Rose Copper. *Ward.* *C.* Fine Copper. *L.* An old Copper Halfpeny, Charles II's Coin. *Harris.*	9.000
Copper in Half-pence. *L.* . .	8.915
Æs; Cuivre. *Petitus.* . . .	8.875
Cuprum. *Bacon.*	8.866
Copper. *Oxf. Soc.* . .	8.843
Cuprum Suecicum. *Fahrenheit.* .	8.834
Id. Japonense. *Fahrenheit.* . .	8.799
Id. Suecicum. *Muschenbr.* .	8.784
Common Copper. *L.* . . .	8.478
BRASS. An old brass gold weight marked XXXIII. *Harris.* . . .	8.830
Aurichalcum. *Bacon.* . . .	8.747
A Piece of hammer'd Brass. *Harris.*	8.660
Æs, Airin, Calaminæ mixtum. *Petitus.*	8.437
Aurichalcum. *Fahrenheit.* . .	8.412
Brass hammer'd. *J. C.* Plate Brass. *Ward.*	8.349
Wrought Brass. *J. C.* . . .	8.280
Cast Brass. *L.*	8.208
D°. *J. C.* *Ward.* . .	8.100
D°. *Cotes.*	8.000
Brass hammer'd. *Reynolds.* . .	7.950
D°. Cast. *Reynolds.* . . .	7.905
A Piece of cast Brass. *Harris.* .	7.666

♂ IRON. Ferrum. *Villalpandus.*	8.086
Id. *Ghetaldus.*	8.000
Iron, forged. *Reynolds.*	7.906
Ferrum. *Petitus.*	7.875
Id. *Bacon.*	7.837
Spanish bar Iron. *L.*	7.827
Swedish D°. *L.*	7.818
Ferrum. *Fahrenheit.*	7.817
Iron. *Cotes.*	7.645
D°. of a key. *J.C.* Common Iron. *Ward.*	7.643
A piece of hammer'd Iron, perhaps part Steel. *Harris.*	7.600
Iron cast. *Reynolds.*	7.520
D°. cast. *L.*	7.135
Softest cast Iron or Dutch Plates. *L.*	6.960
STEEL. *J. C. Ward.*	7.852
D°. *Cotes.*	7.850
D°. Spring Temper. *Hauksbee.*	7.809
D°. Nealed soft. *L.*	7.792
D°. Soft. *Hauksbee.*	7.738
D°. Hard. *Hauksbee.*	7.704
D°. Harden'd. *L.*	7.696
♃ TIN. *Reynolds.*	7.617
Stannum. *Bacon.*	7.520
Id. *Villalpandus. Freind.*	7.500
Etain d'Angleterre. *Musschenbr.*	7.471
Stannum. *Ghetaldus.* $7\frac{2}{5}$	7.400
Id. Provinciæ Indiæ Or. Malacca. *Fahren.*	7.364
Block Tin. *Oxf. Soc. Ward. C.*	7.321
Stannum Anglicanum. *Fahrenheit.*	7.313

Id.

Id. commune. *Petitus.*	. . .	7.312
Id. purum. *Petitus.*	. . . .	7.170
Block or Grain Tin. *L.*	. .	7.156

Notes and Obſervations.

As I thought the uſes that might be made of theſe Tables, either in buſineſs or in philoſophy, would beſt be illuſtrated by a few ſhort notes, I have therefore here occaſionally inſerted ſuch obſervations as occurred to me, whilſt I was reviſing them for the preſs: and as many of theſe related chiefly to the preſent defects of my tables, thoſe I thought would probably be of ſervice, to ſuch as might hereafter take the trouble of improving or correcting them.

As the particulars contained in the Tables were extracted from different books, at different times, and at firſt only intended for my own private uſe, I was not ſolicitous to preſerve one uniform language, but generally ſet down every experiment in my common-place, in the words of the author I took it from: and as I have ſince found, that by a tranſlation I might ſometimes happen not ſo juſtly to repreſent the body intended, I have upon the whole judged it beſt, here alſo to tranſcribe them in the ſame languages in which they were at firſt delivered.

To make experiments of this ſort with a ſufficient degree of accuracy requires a pretty deal of care and pains: and as in ſuch as I have made myſelf, I have found great conveniency in the uſe of decimal weights, preferably to thoſe of the common form,

I would alſo recommend the uſe of ſuch to others, who ſhall pleaſe to employ themſelves in the like enquiries. Thoſe I have provided for myſelf have a *Troy* Ounce for their *integer*, and my leaſt weight is the thouſandth part of that quantity, differing conſequently from the half of a *Troy* Grain only as 24 does from 25, which is inconſiderable ſo far as thoſe ſmall weights are concerned. My four ſmalleſt are reſpectively of 1, 2, 3 and 4 of thoſe thouſandth parts, and together make 10, or an unit of the next denomination, that of the 100th part of an ounce. I then have four others, making 1, 2, 3 and 4 100ths, and together the unit of of the next denomination, or one tenth of an ounce, and ſo on. By theſe I ſave the trouble of reducing the common weights to their loweſt denomination in every experiment, and ſometimes perhaps avoid making miſtakes in that very trifling work.

Whenever two or more original writers nearly concur in their experiments upon any ſubject, the Gravity ſo deduced may be well depended upon. But where they differ remarkably it muſt either be imputed to the unequal gravity of the ſubject itſelf, or to ſome error in the tryals, which may eaſily happen in matters that depend on the obſervation of ſo many minute particulars. All thoſe caſes that ſo ſenſibly differ would well deſerve to be re-examined.

The firſt Table above, that of Metals, as it is compoſed of the moſt perfect and uniform bodies in nature, ſeems capable of being adjuſted with the greateſt preciſion, both with relation to the pure Metals

Metals themselves, and to the several degrees of their mixtures one with another, if experiments in all these cases were but made with a sufficient degree of accuracy.

Gold, in the experiments I have made myself, I could never find to come up to the weight assigned it in some of the former tables, and particularly those I have made upon our own coin, and some others have always remarkably fallen short of the weight assigned to the Standard in those same tables. I have inserted that trial in which I found Guineas to come out best; and I may venture to affirm, that that experiment, in particular, was made with as much accuracy as my instrument was capable of, the Pieces were all washed in soap and water, cleaned with a brush, and the air-bubbles well freed and the like. That experiment is besides abundantly confirmed since, by the exact trials lately made by Mr. *Graham* and Mr. *Ellicot*, which were performed with the greatest care; and the fine Gold also mentioned by the last was chosen and prepared with the greatest curiosity.

It may be observed, that the gold medals of Q. *Eliz.* and Q. *Mary*, quoted by *J. C.* were, without doubt, the large Sovereigns of those Queens, which were of the old Standard of *England*, or of gold appointed to be 23 carats, 3 grains and a half fine: That the *Mentz* Ducat, mentioned by the same, if it was one of those *ad Legem Imperii*, which are always in their own mints affirmed to be fine, come out considerably too light: and that the gold coin of the Commonwealth, and the pistoles of *France*, were like our present gold money of the goodness of 22 carats.

Mercury

Mercury is placed in this table among the Metals, by reaſon of its near agreement with thoſe bodies in its ſpecific gravity; tho' it otherwiſe ſo widely differs from them in moſt of its properties.

Braſs is conſiderably condenſed by hammering; whether Gold, Silver, and the other Metals are alſo condenſed in like manner, hardly appears yet to have been ſufficiently tried.

Of the mixed Metals, hardly any except Braſs, appear to have had their ſpecific gravities very carefully aſcertained: bell-metal, princes metal, however, and ſome others, might deſerve to be examined in that particular.

It might poſſibly be queried alſo, whether ſeveral mixed Metals do not either rarifie or condenſe upon mixture, ſo as thereby to acquire a different ſpecific gravity, than the natural law of their compoſition, at firſt ſeems to require.

It may laſtly be obſerved, that the ſpecific gravities of all the known Metals are ſuch, as that none of them come up to 20 times the weight of common water, or fall ſenſibly below 7 times the ſame weight.

TABLE II.

Of Minerals, Semimetals, Ores, Preparations and Recrements of Metals, &c.

BISMUTH. *J. C.*	9.859
D°. *Cotes*	9.700
D°. or Tinglaſs. *Boyle.*	9.550

Tynglaſs,

Tynglafs. *Reynolds* . . .	7.951
Marcafita alba. *Fahrenheit.* . .	9.850
Mineral, Cornifh, fhining like a Marcafite. *Boyle.*	9.06
Calx of Lead. *Boyle.* . . .	8.940
Spelter Solder. *J. C.* . . .	8.362
Spelter. *J. C.* . . .	7.065
Cinnabar common. *Boyle.* . .	8.020
Cinnabaris factitia. *Mufschenb.* (if not a miftake for the laft experiment) 8.200	
Cinnabar native, breaking in polifh'd furfaces like Talc. *Davies.* . .	7.710
D°. Perfian, breaking rough. *Davies.*	7.600
D°. native. *Boyle.*	7.576
Cinnabaris nativa. *Mufschenb.* . . .	7.300
Cinnabar native, very fparkling. *Boyle.*	7.060
D°. native from Guinea. *Davies.*	6.280
Cinnabar of Antimony. *Harris.*	7.060
D°. another piece. *Harris.* . .	7.043
D°. *Boyle.*	7.030
Cinnabar Antimonii. *Freind.* . .	6.666
Cinnabre d'Antimoine. *Mufschenb.* .	6.044
Lead Ore, rich, from Cumberland. *Boyle.*	7.540
D°. *Boyle.*	7.140
The reputed Silver Ore of *Wales.* *J. C.* .	7.464
The Metal thence extracted. *J. C.* 11.087.	
Regulus Antimonii. Item Martis et Veneris. *Freind.*	7.500
Id. *Fahrenheit.*	6.622
Id. *Harris.*	6.600
Id. per fe. *Davies.*	4.500
Silver Ore, choice. *Boyle.* . .	7.000

D°.

D°. another piece from Saxony. *Boyle.*	4.970
Lithargyrus Argenti. *Freind.*	6.666
Lithargyrium Argenti. *Mufschenb.*	6.044
Id. Auri. *Freind.*	6.316
Id. Auri. *Mufschenb.*	6.000
Minera Antimonii. *Davies.*	5.810
Cuprum calcinatum. *Freind.*	5.454
Glafs of Antimony. *Newton.* C.	5.280
Vitrum Antimonii. *Freind.*	5.000
Id. per fe. *Boyle.*	4.760
Tin Ore, choice. *Boyle.*	5.000
D°. black, rich. *Boyle.*	4.180
New Englifh Tin Ore, Mr. Hubert's. *Boyle.*	4.080
Tutty, a piece. *Boyle.*	5.000
Tutia. *Mufschenb.*	4.615
Lapis Calaminaris. *Freind.* Lapis cæruleus Namurcenfis. *Mufschenb.*	5.000
Id. *Boyle.*	4.920
Loadftone. *Boyle* V. 6. *b.*	4.930
Magnes. *Petitus.*	4.875
A good Loadftone. *Harris.*	4.750
Marcafites, one more fhining than ordinary. *Boyle.*	4.780
A Golden Marcafite. *J. C.*	4.589
Marcafites, from Stalbridge. *Boyle.*	4.500
D°. *Boyle.*	4.450
Antimonium Hungaricum. *Mufschenbr.*	4.700
Antimony, good, and fuppofed to be Hungarian. *Boyle.*	4.070
D°. crude, which feemed to be very good. *Harris.*	4.058

Antimonium

Antimonium crudum. *Freind.*	4.000
Id. *Davies.*	3.960
Black Sand, commonly uſed on writing. *Boyle.* V. 33. *b.*	4.600
Crocus Metallorum. *Muſchenb.*	4.500
Id. *Freind.*	4.444
Hæmatites. *Muſſchenbr.*	4.360
Id. *Boyle.* V. 6. *a.*	4.150
D°. Engliſh. *Boyle.*	3.760
Copper Ore, rich. *Boyle.*	4.170
D°. *Boyle.*	4.150
Copper-ſtone. *Boyle.*	4.090
Emeri. *Boyle.* V. 26. *b.*	4.000
Mangancſe. *Boyle.*	3.530
A blew Slate with ſhining particles. *J. C.*	3.500
Iron Ore, a piece burnt or roaſted. *Harris.*	3.333
Ceruſſa. *Item* Chalybs cum Sulphure. pp. *Freind.*	3.158
Lapis Lazuli. *J. C.*	3.054
D°. *Boyle.* V. 6. *b.*	3.000
D°. *Boyle.*	2.980
Gold Ore. *Boyle.* V. 29. *b.*	2.910
D°. not rich, brought from the Eaſt Indies. *Boyle.*	2.652
Another Lump of the ſame. *Boyle.*	2.634
A Mineral Stone, yielding 1 part in 160 Metal. *J. C.*	2.650
The Metal thence extracted. *J. C.* 8.509.	
Pyrites homogenea. *Fahrenheit.*	2.584
Black Lead. *Boyle.* V. 27. *a.*	1.860
Æs viride. *Freind.*	1.714
Plumbum uſtum. *Freind.*	1.666

The ſecond Table is compoſed of ſubjects no way ſtrictly allied to each other, either by their gravities, or their other eſſential properties; and perhaps they might better, on that account, have been divided into different tables.

The bodies themſelves are chiefly of an uncertain and heterogeneous nature; being ſo far as appears compoſed of different elements, and thoſe alſo combined in various proportions, ſuch as Sulphur and Arſenic, joined with Stone, Metal, and the like: and from theſe ſeveral degrees of mixture it muſt follow, that moſt of theſe kinds of bodies, tho ſo far ſimilar as to be called by the ſame names, yet muſt neceſſarily admit of a conſiderable latitude in their ſpecific gravities. Many uſeful deductions may nevertheleſs be drawn from thoſe conſiderations, relating to the comparative goodneſs &c. of ſuch bodies.

Cinnabar native appears to be a compound of Mercury and Sulphur, with a portion of earthy or ſtony matter; and that which is heavieſt muſt abound moſt with the Mercury. The different appearances which this body makes, would alſo give us a ſuſpicion that there are other varieties in its compoſition, beſides thoſe juſt taken notice of: ſome ſorts of Cinnabar, ſuch as the *Hungarian*, breaking into poliſhed planes and ſquares like Talc, whilſt others, like the *Perſian* of this table, break rough and with ſhining *granulæ* or *micæ*; and that without any conſiderable difference in their gravities.

By the factitious Cinnabar it may be determined, what proportion of Mercury will ſo incorporate with Sulphur, as to make up an uniform body.

Antimony

Antimony may in like manner be confidered as a compofition of its Regulus and Sulphur.

The black fand ufed on writing is faid by Mr. *Boyle* to be a rich Iron Ore: he alfo fays that Emeri, Loadftone, and all fuch ponderous ftones, contain fome kind of metal, which he had himfelf feparated from them. IV. 120. *a.*

The great variety of Ores of all kinds well deferve to be accurately examined, for the fake of the many conclufions that may be drawn from thence, concerning the natures of concrete bodies, and for many other purpofes in Metallurgy. But I have as yet met with a very fmall number of experiments upon thefe fubftances. Dr. *Woodward* has indeed mentioned a great many obfervations of this fort which he had made, and kept exact regifters of: but as they were probably among thofe papers which he order'd to be deftroy'd at his death, we muft look upon them as now loft to the world.

The Marcafites and Pyrites are very uncertain and ftrange kinds of bodies, their gravities are often very great: a Marcafite here taken from *Fahrenheit* was found nearly to equal the heavieft mineral Bifmuth itfelf; and yet it is very feldom that any Metal or femimetal can be obtained in any quantity from thefe fubftances, all that is in them being ufually deftroyed, and carried away by their fulphur.

Black Lead is alfo a very odd kind of Mineral, having all the appearance of a Semimetal, and yet falling fhort even of the weight of common earth.

The Semimetals generally exceed in their fpecific gravity even the bafer Metals themfelves.

It may be obſerved, that it appears by this table, that the ſpecific gravities of ores, including the metallic ſtones, are uſually found to lie between 7 and 3 times the weight of water. Lead and Silver ores are the heavieſt, thoſe of Copper, Tin and Iron being conſiderably lighter. The Gold Ore we have an account of muſt be ſo poor as hardly to be worth taking any notice of: but we have in general too few of theſe experiments, to draw any certain concluſions from them.

TABLE III.

Of Gems, Chryſtals, Glaſs, and tranſparent Stones.

GRANATE, Bohemian. *Boyle.* . .	4.360
Granate. *J. C.*	3.978
Granati minera. *Boyle.* . . .	3.100
A Pſeudo-Topazius, being a natural pellucid, brittle, hairy ſtone, of a yellow colour. *Newton. C.*	4.270
Sapphires. *Davies.*	4.090
A Sapphire very perfect, but rather pale. *Hauksbee.*	4.068
Glaſs, blue in ſticks from Mr. Seale. *Hauksbee.*	3.885
D°. whiteſt, from Mr. Seale. *Hauksbee.*	3.380
D°. clear chryſtal. *Cotes.* . .	3.150
D°. blue plate, old. *Hauksbee.* .	3.102
D°. plate. *L.* . . .	2.942

D°.

Dº. old looking-glaſs plate of a light colour. *Hauksbee.* . . .	2.888
Dº. green. *Freind.* . . .	2.857
Dº. green bottle. *Hauksbee.* . .	2.746
Dº. of a bottle. *Oxf. Soc.* It. a blue paſte. *Hauksbee.* . . .	2.666
Dº. common green. *Hauksbee.* .	2.620
Dº. deep green old. *Hauksbee.* .	2.587
Dº. vulgar. *Newton. Ward.* . .	2.580
Vitrum Venetum. *Freind.* . .	1.791
An oriental Cat's-Eye, very perfect. *Hauksb.*	3.703
A Diamond, yellow, of a fine water, ſomewhat paler than the jonquille. *Hauksbee.*	3.666
Dº. white of the ſecond water. eau celeſte. *Hauksbee.*	3.540
Dº. Eaſt Indian, the heavieſt of many. *Ellicot.*	3.525
Dº. the lighteſt of many. *Ellicot.* . .	3.512
Dº. Braſilian, the heavieſt of many. *Ellicot.*	3.521
Dº. the lighteſt of many. *Ellicot.* . .	3.501
Dº. the mean of all his experiments. *Ellic.*	3.517
Dº. *Newton. C.*	3.400
Diamond Bort, of a bluiſh black, with ſome little adhering foulneſs. *Hauksbee.*	3.495
A Jacinth of a fine colour, but ſomewhat foul. *Hauksbee.* . . .	3.637
A Chryſolite. *Hauksbee.* . . .	3.360
Chryſtal cubic, ſuppoſed to contain lead. *Woodward.*	3.100
Chryſtal from Caſtleton in Derbyſhire, having the double refraction. *Hauksbee.*	2.724
Chryſtal of Iſland. *Newton. C.* . .	2.720

Chryſtallum

Chryſtallum diſdiaclaſticum. *J. C.*	2.704
Chryſtallus de Rupe. *Fahrenheit.*	2.669
Chryſtal rock. *J. C.* *Boyle* III. 229. *b.*	2.659
Do. a large ſhoot. *Hauksbee.*	2.658
Do. of the rock. *Newton.* *C.* It.	
Chryſtal in the lead-mines near Works-worth. *Woodward.*	2.650
Do. *Hauksbee.*	2.646
Do. pure pyramidal, ſuppoſed to contain Tin. *Woodward.* 2. 5 or	2.400
Chryſtallus. *Petitus.*	2.287
Chryſtal. *Boyle.*	2.210
Talc, Jamaican. *Boyle.*	3.000
Do. Venetian. *Boyle.*	2.730
Do. *J. C.*	2.657
Do. Engliſh. *Woodward.*	2.600
Do. a piece like Lapis Amianthus. *Boyle.*	2.280
A red paſte. *J. C.*	2.842
A Braſile pebble, foul and feather'd. *Hauksb.*	2.755
Do. a fragment uncut. *Hauksbee.*	2.676
Do. cut. *Hauksbee.*	2.591
Jaſper, ſpurious. *J. C.*	2.666
A Corniſh Diamond cut. *Hauksbees*	2.658
A Water Topaz, very perfect, but ſaid not to be Oriental. *Hauksbee.*	2.653
Pebble pellucid. *J. C.*	2.641
Briſtol Stone. *Davies.*	2.640
Hyacinth, ſpurious. *J. C.*	2.631
Selenites. *J. C.*	2.322
Do. *Newton.*	2.252

As the mean gravity of Chryſtal appears, by the foregoing table, to be little more to that of water than

than as two and a half to one; it may well be ſuſpected, that the Granate, Pſendo-Topazius, Sapphire, and ſuch other Gemms which greatly exceed Chryſtal in weight, do contain a conſiderable portion of ſome ſort of Metal in their compoſition: as was obſerved of theſe bodies by Dr. *Woodward*, in his Method of Foſſils, p. 24.

As to the white Sapphire, which is reputed by Dr. *Woodward* to be a ſpecies of Gemm intermediate between Chryſtals and the Diamond in hardneſs, I have not yet obtained any good account of its ſpecific gravity.

The weight of the Diamond is aſcertained in N°. 476. of the *Philoſophical Tranſactions*, where it appears, that by experiments made with the greateſt care, by Mr. *John Ellicot* F.R.S. with moſt exact inſtruments, and upon 14 different Diamonds, ſome of them very large, brought from different places, and having the greateſt varieties of colour and ſhape poſſible; they were all found to agree in weight to a ſurpriſing degree of accuracy, being all ſomewhat above three times and a half the weight of common water.

This indeed differs very ſenſibly from what had been found in ſome former experiments, but it is hardly probable that thoſe had been made upon Diamonds of ſo large a ſize as theſe: Mr. *Boyle* who found their weight leſs than 3 times that of common water, has himſelf told us in the ſame place, V. 83. *b.* that the ſtone he made uſe of only weighed about 8 grains. And tho no doubt can be made of the exactneſs of Sir *Iſaac Newton's* expe-

riment,

riment, by which alſo the ſpecific weight of the Diamond came out leſs than Mr. *Ellicot's*, yet it may well be queſtion'd, whether Sir *Iſaac* had, at the time when he made his trials, either ſo many or ſo perfect and weighty ſtones, as a favourable opportunity offered to this laſt gentleman. I ſhall therefore only obſerve, that, admitting this laſt to be the true ſpecific weight of the Diamond, the refractive power of the ſame, in proportion to its denſity, ſhould in Sir *Iſaac Newton's* table be leſſened from 14556 to 14071; which would ſtill be greater than what is found in any other body; but is upon the whole more conformable to the general law of that table.

Sir *Iſaac Newton* conjectured a Diamond to be an unctuous ſubſtance coagulated, and found it to have its refractive power nearly in the ſame proportion to its denſity as thoſe of Camphire, Oyl-Olive, Lintſeed Oyl, Spirit of Turpentine and Amber, which are fat ſulphureous unctuous bodies: all which have their refractive powers two or three times greater in reſpect to their denſities than the refractive powers of other ſubſtances in reſpect of theirs. Yet muſt it be allowed that a Diamond ſuffers no change by heat in any degree, contrary to the known property of Sulphurs; and as it is moſt reaſonable in our Philoſophy to treat ſuch bodies as ſimple, in which we are not able to produce any change or ſeparation of parts, we muſt therefore on that account conſider a Diamond as a ſimple body and of the Chryſtalline kind.

Glaſs, which is a factitious concrete of Sand and Alkaline Salt, is nearly found to aſſume the mean gravity of Stones and Chryſtals.

If,

If there is no miſtake in the gravity of what Dr. *Freind* calls *Vitrum Venetum*, it differs very remarkably from all other kinds of Glaſs.

I do not know whether the Jaſper and Hyacinth ſpurious of *J.C.* are to be underſtood as natural or artificial Gemms.

TABLE IV.

Of Stones and Earths.

Sardachates. *J. C.*	3.598
Lapis ſciſſilis cæruleus. *Muſschenbr.* (qu. if not the ſame experiment mentioned before pag. 447. *a blew ſlate with ſhining particles. J. C.*)	3.500
Cornelian. *Boyle.*	3.290
Dº. *J. C.*	2.563
A Hone. *J. C.*	3.288
Dº. to ſet razors on. *Harris.*	2.960
Marmor. *Petitus.* (probably ſome miſtake in the experiment.) 3.937.	
Marble. *Reynolds.*	3.026
Dº. white. *Hauksbee.*	2.765
Dº. white Italian, of a cloſe texture viſibly.	2.718
Dº. white. *Boyle.* fine. *Ward. C.*	2.710
Dº. white Italian, tried twice. *Oxford Soc.*	2.707
Dº. black Italian. *Oxford. Soc.* veined. *L.*	2.704
Dº. black. *Hauksbee,*	2.683
Dº. Parian. *L.*	2.560
Lapis Amianthus, from Wales. *J. C.*	2.913
Turquoiſe, one of the old rock, very perfect. *Hauksbee.*	2.908
Turcoiſe Stone. *J. C.*	2.508

Lapis Nephriticus. *J. C.*	2.894
Corallium rubrum. *Freind.*	2.857
Corall. *J. C.*	2.689
Do. red. *Boyle* V. 7. *a.*	2.680
Do. *Boyle.*	2.630
Do. white, a fine piece. *Boyle.*	2.570
Do. white, another piece. *Boyle.*	2.540
Emeril Stone, a ſolid piece. *Hauksbee.*	2.766
Paving Stone. *Reynolds.*	2.708
Do. a hard ſort from about Blaiden. *Oxf. Soc.*	2.460
A Whetſtone, not fine, ſuch as cutlers uſe. *Harris.*	2.740
Pellets, vulgarly called Alleys, which boys play withal. *Hauksbee.*	2.711
Engliſh Pebble. *L.*	2.696
Lapis Judaicus. *Boyle.*	2.690
Id. *Freind.*	2.500
Maidſtone Rubble. *L.*	2.666
Marbles, vulgarly ſo called, which boys play withal. *Hauksbee.*	2.658
Morr Stone. *L.*	2.656
Agate. *Boyle.*	2.640
Do. German, for the lock of a gun. *Hauksbee.*	2.628
Do. Engliſh. *J. C.*	2.512
Lapis, *Petitus.*	2.625
Flint, black, from the Thames. *Hauksbee.*	2.623
Flint Stone. *L.*	2.621
A round pebble-ſtone within a flint. *Harris.*	2.610
Eaſt Indian blackiſh. Item, an Engliſh one. *Boyle.* III. 243. *a.*	2.600

Do.

Do. *Oxford Soc.* . . .	2.542
Corallachates. *J. C.* . . .	2.605
Purbeck Stone. *L.* . .	2.601
Freeſtone. *Reynolds.* . .	2.584
Portland Stone. *L.* . .	2.570
Do. white for carving. *L.* .	2.312
Grammatias Lapis. *J. C.* . .	2.515
Onyx Stone. *J. C.* . . .	2.510
Slate Iriſh. *Boyle.* Lapis Hibernicus. *Davies.* . . .	2.490
Wood petrified in Lough Neagh. *J. C.*	2.341
Oſteocolla. *Boyle.* . . .	2.240
Heddington Stone. *L.* . .	2.204
Allom Stone. *Boyle.* . . .	2.180
Bolus Armena. *Freind.* . .	2.137
Hatton Stone. *L.* . . .	2.056
Burford Stone, an old dry piece. *Oxford Soc.* . . .	2.049
Heddington Stone, that of the ſoft lax kind. *Oxford Soc.* . .	2.029
Terra Lemnia. *Freind.* . .	2.000
Brick. *Cotes.* . .	2.000
Do. *Oxford Soc.* . .	1.979
A Gallypot. *J. C.* . .	1.928
Alabaſter. *Ward. C.* . .	1.874
Do. *Oxford Soc.* . .	1.872
A ſpotted factitious Marble. *J. C.* .	1.822
Stone Bottle. *Oxford Soc.* . .	1.777
A piece of a glaſs (perhaps glazed) coffee-diſh of a brown colour. *Harris.* .	1.766
Barrel Clay. *L.* . .	1.712
Lapis de Goa. *Davies.* . .	1.710
Lapis ruffus Bremenſis. *Muſchenb.* .	1.666

An Icicle broken from a Grotto (I ſuppoſe Stalactites) *Dr. Slare*, in *Harris*.	1.190
Chalk, as found by Dr. *Slare*. *Harris*.	1.079

The mean gravity of Stone appears to be to that of water as about two and a half to one, and many ſtones of great hardneſs, ſuch as the Onyx, Turquoiſe, Agat, Marble, Flint *&c.* do not much exceed that weight. It may therefore well be doubted whether ſuch Stones whoſe ſpecific gravity comes up to near three times that of water, or even beyond it, owe their denſity to metalline additions; or whether they are really formed of a different ſpecies of matter, as the Diamond ſeems to be.

Coral by its denſity appears to be a ſtone, tho in a vegetating ſtate: or it may poſſibly from ſome late obſervations, be of an animal nature.

What is called *Lapis Hibernicus*, is a ſoft ſtone containing Vitriol.

We have not many obſervations upon Earths: by thoſe we have, it ſeems probable that they contain the ſame kind of matter in a lax form, of which Stones are a more ſolid and denſer concretion.

Lapis de Goa is but a trifling compoſition, perhaps hardly worth retaining in the tables.

What ſpecies of body ſhould *Alabaſter* be accounted? which with a ſtone-like hardneſs, yet falls ſo much below other Stones, or even Earths, in gravity.

TABLE V.

Of Sulphurs and Bitumens.

SULPHUR. *Petitus.*	2.344
D°. a piece of roll. *Hauksbee.*	2.010
D°. vive. *Boyle.*	2.000
D°. German, very fine. *Boyle.*	1.980
D°. tranſparent, Perſian. *Davies.*	1.950
Sulphur mineralis. *Freind.*	1.875
Brimſtone, ſuch as is commonly ſold. *J. C.*	1.811
D°. *Cotes.*	1.800
Aſphaltum. *Boyle.* III. 243. *a.*	1.400
Scotch Coal. *Boyle.* III. 243. *a.*	1.300
Coal, of Newcaſtle. *L.*	1.270
D°. Pit, of Staffordſhire. *Oxford Soc.*	1.240
Jet. *J. C.*	1.238
D°. *Davies.*	1.160
D°. *Davies.*	1.020
Succinum citrinum. *Davies.*	1.110
Id. pingue. *J. C.*	1.087
Id. flavum (by 2 experiments). *Davies.*	1.080
Id. pellucidum. *J. C.*	1.065
Id. album, item pingue. *Davies.*	1.060
Amber. *Boyle. Newton. C.*	1.040
Fine Gunpowder. *Reynolds.*	0.698

Sulphur is in gravity very nearly the ſame as Earth, ſo that its purity can hardly be aſcertained by its weight, unleſs the matter it is aſſociated with is of a ſtony denſity.

The

The ſemidiaphanous *Sulphur* is a beautiful kind which I have but ſeldom ſeen: it is in lumps of the ſize of a ſmall bean.

Coal, the ſorts here taken notice of are conſiderably lighter than *Sulphur*: but there are many other kinds, and of different weights.

I take the *Gagates* or *Jet* to differ very little from the *Channel Coal*.

The different ſorts of *Amber* may be obſerved not to differ conſiderably in their ſeveral gravities.

Sulphurs ſeem to be the lighteſt of all mineral bodies.

TABLE VI.

Of Gums, Reſins, &c.

GUM Arabic. *Freind.*	1.430
D°. *Newton. C.*	1.375
Opium. *Freind.*	1.360
Gum Tragacanth. *Freind.*	1.330
Myrrh. *Freind.*	1.250
Gum Guaiac. *Freind.*	1.224
Reſina Scammonii. *Freind.*	1.200
Aloes. *J. C.* (qu. whether the reſin or the wood).	1.177
Aſa fœtida, a very fine ſample. *Hauksbee.*	1.251
D°. from *Dr. John Keill's Introd. ad veram Phyſicam.*	1.143
Pitch. *Oxford Soc. C.*	1.150
Thus. *Freind.*	1.071
Camphire. *Newton. C.*	0.996
Bees-wax. *Cotes.*	0.955

Cera.

Cera. *Ghetaldus.* (ad aquam ut $95\frac{5}{11}$ ad 100).	0.954
Wax well freed from the honey. *Davies.*	0.938
Cera. *Petitus.* . . .	0.937
D°. the ſame lump 2 years after. *Davies.*	0.942
Balſamus de Tolu. *Muſschenbr.* .	0,896
Maſtic. *J. C.* (qu. whether the gum or the wood). . . .	0.849

The bees wax in my own experiments was well freed from honey, by the boyling it in water, which probably made it lighter than it was ſet down in Mr. *Cotes*'s Table: and the ſecond experiment which I made two years after the firſt, if the difference was not owing to the difference of heat, is an inſtance of what I take to be a pretty general truth, that bodies become more denſe and compact by reſt, and that they would alſo be found heavier in the ſcale, in thoſe caſes where they do not loſe weight by the evaporation of humidity.

The weights of vegetable Gums nearly correſpond with thoſe of the ligneous parts.

TABLE VII.

Of Woods, Barks &c.

COCO Shell. *Boyle.* . .	1.345
Bois de Gayac. *Muſschenbr.*	1.337
Lignum Guajacum. *Freind.* . .	1.333
Lignum Vitæ. *Oxf. Soc.* . .	1.327
Speckled Wood of Virginia. *Oxf. Soc.* .	1.315
Cortex Guaiaci. *Freind.* . . .	1.250

Lignum

Lignum Nephriticum. *Freind.*	1.200
Lignum Asphaltum. *J. C.*	1.179
Ebony. *J. C.* *Item* Aloes. *J. C.*	1.177
Santalum rubrum. *J. C.*	1.128
Id. album. *J. C.*	1.041
Id. Citrinum. *J. C.*	0.809
Lignum Rhodium. *J. C.*	1.125
Radix Chinæ. *Freind.*	1.071
Dry Mahogany. *L.*	1.063
Gallæ. *Freind.*	1.034
Red wood. *Oxf. Soc.* It. Box wood. *Oxf. Soc. Ward. C.*	1.031
Log wood. *Oxf. Soc.*	0.913
Oak, dry, but of a very sound close texture. *Oxf. Soc.*	0.932
D°. tried another time. *Oxf. Soc.*	0.929
D°. sound dry. *Ward.*	0.927
D°. dry. *Cotes.*	0.925
D°. dry, English. *L.*	0.905
Oak of the outside sappy part, fell'd a year since. *Oxf. Soc.*	0.870
D°. *Reynolds.*	0.801
D°. very dry, almost worm-eaten. *Oxf. Soc.*	0.753
Dry Wainscot. *L.*	0.747
Beech meanly dry. *Oxf. Soc.*	0.854
Mastic (qu. if the wood or gum). *J. C.*	0.849
Ash dry about the heart. *Oxf. Soc.*	0.845
D°. dry. *Cotes.*	0.800
D°. meanly dry, and of the outside lax part of the tree. *Oxf. Soc.*	0.734
Elm dry. *L.*	0.800
D°. *Reynolds.*	0.768

D°.

D°. *Oxf. Soc. C.* . . .	0.600
Rad. Gentianæ. *Freind.* . . .	0.300
Cortex Peruvianus. *Freind.* . .	0.734
Crabtree meanly dry. *Oxf. Soc.* .	0.765
Yew, of a knot or root 16 years old. *Oxf. Soc.*	0.760
Maple dry. *Oxf. Soc. C.* . .	0.755
Plumtree dry. *J. C.* . . .	0.663
Fir, dry yellow. *L.* . . .	0.657
Dry white Deal. *L.* . . .	0.569
Lignum Abietin. *Freind.* . .	0.555
Fir dry. *Cotes.*	0.550
D°. *Oxf. Soc.*	0.546
Walnut tree dry. *Oxf. Soc.* . .	0.631
Cedar dry. *Oxf. Soc.* . . .	0.613
Juniper wood dry. *J. C.* . .	0.556
Saſſafras wood. *J. C.* . . .	0.482
Cork. *Cotes.*	0.240
D°. *J. C.*	0.237

Dr. *Jurin* has obſerved in the *Phil. Tranſ.* N°. 369. that the ſubſtance of all wood is ſpecifically heavier than water, ſo as to ſink in it, after the air is extracted from the pores and air-veſſels of the wood, by placing it in warm water under the receiver of an air-pump; or if an air-pump cannot be had, by letting the wood continue ſome time in boiling water over a fire. The ſeveral weights therefore above given muſt be looked upon as the weights of the concrete bodies, in the condition they were, before the Air was either forcibly got out, or the water driven into the ſmall hollows: and both theſe conſiderations may have their uſe

as notwithſtanding that the ſpecific weights of the ſolid particles are truly heavier than water, we ſhall from the weights of the bodies as they are now compounded, be enabled to make ſome judgment of their poroſity, ſo far as they may be penetrable by water or other fluids.

TABLE VIII.

Of Animal Parts.

MANATI Lapis. *Boyle.*	2.860
D°. another. *Boyle.*	2.330
D°. a fragment of. *Boyle.*	2.290
D°. *J. C.* another from Jamaica. *Boyle.*	2.270
Pearl, very fine Seed, oriental. *Boyle.* V. 12. *a.*	2.750
D°. a large one, weighing 206 grains. *Boyle* V. 7. *b.*	2.510
Murex Shell. *J. C.*	2.590
Crabs Eyes artificial. *Boyle.*	2.480
D°. native. *Boyle.*	1.890
Os ovinum recens. *Freind.*	2.222
Oyſter Shell. *J. C.*	2.092
Calculus humanus, juſt voided. *Davies.*	2.000
D°. *Boyle.* V. 7. *b.*	1.760
D°. *Boyle.*	1.720
D°. *Cotes.*	1.700
D°. *Boyle.* V. 7. *b.*	1.690
D°. *J. C.*	1.664
D°. *Davies.*	1.650
D°. *Boyle.*	1.470

D°.

D°. *J. C.*	1.433
D°. *Davies.*	1.330
D°. *J. C.*	1.240
Rhinoceros Horn. *Boyle.* . . .	1.990
The top part of one. *J. C.* . .	1.242
Ebur. *Freind.*	1.935
Ivory. *Boyle.* . . .	1.917
D°. dry. *Oxford Soc. C.* . .	1.826
D°. *Ward.*	1.823
Unicorn's Horn, a piece. *Boyle.* . .	1.910
Cornu Cervi. *Freind.* . . .	1.875
Ox's Horn, the top part of one. *J. C.* .	1.840
Blade bone of an Ox. *J. C.* . .	1.656
A ſtone of the Bezoar kind found with four others in the inteſtines of a mare. *Edw. Bailey M.D.* of *Havant* in *Hampſhire.* See Philoſoph. Tranſact. No. 481.	1.700
Bezoar ſtone. *Boyle.*	1.640
D°. a large one. *Davies.* . .	1.570
D°. being the kernel of another. *Boyle.* V. 8. *a.*	1.550
D°. a fine oriental one. *Boyle.* .	1.530
D°. two weigh'd ſeparately. *Davies.* .	1.504
D°. *Cotes.*	1.500
D°. *Boyle.* . . .	1.480
D°. *Boyle.*	1.340
A ſtone from the Gall-bladder. *Hales.* .	1.220
Blood human, the globules of it. *Jurin by calculation.* . . .	1.126
D°. the Craſſamentum of. *Jurin from Experiments.* . .	1.086
D°. *Davies.*	1.084

D°. from another Experiment. *Jurin.*		1.082
Sanguinis humani cuticula alba. *Davies.*		1.056
Human blood when grown cold. *Jurin.*		1.055
The same as running immediately from the vein. *Jurin.*	. . .	1.053
The serum of human blood. *Jurin.*	.	1.030
D°. *Davies.*	. . . .	1.026
Ichthyocolla. *Freind.*	. . .	1.111
A Hen's Egg. *Davies.*	. . . .	1.090
Milk. *J. C. C.*	. . . .	1.031
Lac caprinum. *Muschenbr.*	. .	1.009
Lac. *Freind.*	. . . . .	0.960
Urine. *J. C. C.*	. . .	1.030
Id. *Freind.*	. . .	1.012

Manati Lapis is said to be a stone, found in the head of the Manatee, or Sea-Cow of the *West-Indies*. See *Ray's Synopsis methodica Animalium Quadrupedum &c. Lond.* 1693. 8°. These Stones and Pearls are the heaviest of all the animal productions we are acquainted with.

Dr. *Jurin* has observed, *Phil. Trans.* N°. 369. that, in examining fresh Human *Calculi* whilst they were still impregnated with Urine, he had met such as exceeded the weight of some sorts of burnt earthen ware and alabaster, and approached very near to that of brick, and the softer sort of paving stone; which I have myself also found to be true. Whereas those who have made their experiments upon such *Calculi*, as had most probably been a considerable time taken out of the bladder, and had consequently lost much of their weight, by the evaporation of the urine, with which they had at first

firſt been ſaturated, have found thoſe Stones commonly to have been but about one half part, and ſome of them no more than a fourth part, heavier than an equal bulk of Water. From whence it has been too haſtily concluded, that theſe Stones have very improperly been called by that name, as not at all approaching to the Specific Gravity of even the lighteſt real ſtones that we have any account of.

The *Calculus Humanus* and *Animal Bezoar* approach nearly to each other in their Specific Gravity.

Mr. *Boyle* has taken notice of the great difference to be found between the gravity of the true and the factitious Crabs-eyes. It is ſtrange that the factitious ſhould be made of ſuch materials as can bring them ſo near to the mean gravity of true ſtones: and this conſideration may deſerve the attention of thoſe, who may think that any praticular dependence is to be had upon the uſe of theſe bodies in medicine.

Dr. *Jurin* was the firſt who carefully examined the Specific Gravities of the different parts which compoſe Human Blood; and his experiments were performed with the greateſt accuracy. It may be obſerved, that the Blood is, by an eaſy *analyſis* divided into *Serum* and *Craſſamentum*; and the *Craſſamentum* again into the Glutinous and the Red globular parts, whoſe Specific Gravities are the greateſt. It had before theſe experiments been the general received opinion, that the globules of the Blood were lighter than the Serum; and this indeed ſeemed to follow from Mr. *Boyle*'s Experiments in his Natural Hiſtory of *Human Blood*; from which he deduced the Specific Gravity of the maſs itſelf; to be to that of

of Water as 1040 to 1000, and that of the Serum alone to be to the ſame as 1190. And theſe numbers 1040 and 1190 had accordingly, till Dr. *Jurin* re-examined the affair, been conſtantly taken to repreſent the true gravities of Human Blood and its Serum reſpectively. See Dr. *Jurin's diſſertation* in *Phil. Tranſ.* N°. 361.

Milk is made by Dr. *Freind* to fall more ſhort of the Gravity of Water, than it is made to exceed the ſame by *J. C.* Poſſibly this difference might ariſe from the Milk's being taken in one caſe warm from the cow, and in the other after it had ſtood ſome time.

TABLE IX.

Of Salts.

MERCURIUS dulcis bis ſublim. *Muſch.*	12.353
Mercurius dulcis. *Freind.* . .	11.715
Id. ter ſublim. *Muſchenbr.* . .	9.882
Id. tertio ſublim. Item Panacea rubra. *Freind.*	9.372
Id. quater ſublim. *Muſchenbr.* Item Turpethum minerale. . .	8.235
Id. 4*to* ſublim. Item Turpeth mineral. *Freind.*	7.810
Sublimat. corroſiv. *Muſchenbr.* .	8.000
Id. *Freind.*	6.045
Cinis clavellatus, ſordibus ſaleque ſuo neutro quodam (quod fere ſemper magis vel minus in cinere illo reperitur) depurgatus. *Fahrenheit.*	3.112
Sal illud neutrum. *Fahrenheit.* .	2.642

Saccharum Saturni. Item ſal Nitri fix. *Muſchenbr.* . . .	2.745
Eadem. *Freind.* . . .	2.600
Magiſterium Coralli. Item Pulvis ſympatheticus. *Freind.* . . .	2.231
Tartarum vitriolatum. *Muſchenbr.* .	2.298
Id. *Freind.* . . .	2.186
Sal mirabile Glauberi. *Muſchenbr.* .	2.246
Id. *Freind.* . . .	2.132
Tartarum emeticum. *Muſchenbr.* .	2.246
Id. *Freind.* . . .	2.077
Sal Gemmæ. *Newton. C.* . .	2.143
Nitrum. *Fahrenheit.* . .	2.150
Nitre. *Newton. C.* . .	1.900
Id. *Freind.*	1.671
Sal Guaiaci. Item Sal enixum. Item Sal prunellæ. Item S. Polychreſt. *Muſch.*	2.148
Eadem omnia. *Freind.* . .	2.030
Sal maritimum. *Fahrenheit.* . .	2.125
Cremor Tartari. Item Vitriol. alb. Item Vitriol. rubefact. Item S. Vitriol. *Muſch.*	1.900
Cremor Tart. Item Vitriol. alb. *Freind.*	1.796
Vitriol Engliſh, a very fine piece. *Boyle.*	1.880
D°. Dantzick. *Newton. C.* . .	1.715
Alumen. *Fahrenheit.*	1.738
Alum. *Newton.* . . .	1.714
Sal chalybis. *Freind.* . .	1.733
Borax. *J. C.*	1.720
D°. *Newton. C.*	1.714
Vitriolum viride. Item Calcanth. rubefact. Item S. Vitriol. alb. *Freind.* . .	1.671
Saccharum albiſs. *Fahrenheit.* . .	$1.606\frac{1}{2}$
Mel. *Villalpandus.* . . .	1.500
Id. *Ghetaldus* $1\frac{9}{20}$. Honey, *Cotes.* .	1.450

Sal

Sal volatile Cornu Cervi. *Muſchenb.* .	1.496
Id. *Freind.*	1.421
Sal Ammoniac. purum. Item Ens Martis ſemel ſublimat. *Muſchenb.* . . .	1.453
Eadem. *Freind.*	1.374
Ens Martis ter ſublimat. *Muſchenb.* .	1.269
Id. *Freind.*	1.233

Moſt of the experiments in the ninth table are taken from Dr. *Freind*, who weigh'd the Salts in Spirits of Wine, and regiſter'd the proportional gravity of the Salts to the Spirits. But the misfortune is, that the gravity of the Spirits of Wine he made uſe of is not regiſter'd: ſo that the experiments cannot with certainty be reduced to the common ſtandard of Water. He has deliver'd the gravity of Spirits of Wine to be 0.818, and that of Spirits of Wine rectified to be 0.78. I have ſuppoſed the Salts to be weighed in the laſt, as being the fitteſt for the purpoſe: but which he really uſed can only be conjectured.

There appears indeed to be a way to diſcover the weight of the Spirits of Wine, in which Dr. *Freind* weighed his Salts: for he weighed 60 Grains of Mercury, both in Water and in Spirits of Wine, and the loſs of its weight was reſpectively $4\frac{1}{4}$ Grains and $2\frac{2}{3}$. Now the gravities of theſe Fluids muſt be in the ſame proportion, and this would give for the weight of the Spirits of Wine 0.627, which is much too little for the weight of his own rectified Spirits tho even that is leſs than what is aſſigned by any other author. So that, upon the whole, nothing can really be concluded from this experiment; and it

muſt

muſt be allowed beſides, that 60 Grains of Mercury take up too ſmall a bulk in theſe Fluids, to have their gravities determined with any exactneſs thereby.

As Profeſſor *Muſschenbroek* has given in his table the ſpecific weights of many of the ſame ſalts which are mentioned by Dr. *Freind*, but which differ conſiderably from the weights above ſet down, as reſulting from the Doctor's experiments, I have alſo tranſcribed the Profeſſor's numbers from his own table. Theſe do not however appear to me to be derived from new or differing experiments, but from the very ſame related by Dr. *Freind*, only computed from the ſuppoſition of a heavier ſort of Spirits of Wine, whoſe ſpecific gravity is ſuppoſed to have been 0.823. The gravity of the *Sublimate corroſive*, ſet down 8.000, I take to be a miſtake, made by the writing down its comparative weight to that of the Spirits themſelves, inſtead of the water to which it ſhould have been referred.

It requires great care and attention to take the Specific Gravities of Salts with ſufficient accuracy. They diſſolve in Water, and in ſome degree in all Fluids that partake of the nature of Water. If therefore Spirits of Wine are made uſe of for this purpoſe, they ought to be highly rectified, their own gravity accurately aſcertained, and their degree of heat ſhould be preſerved uniform. For as this Fluid rarefies much faſter than Water does, a ſmall difference of heat would ſenſibly affect the gravities of the Salts to be determined by it. And perhaps Spirit of Turpentine were a more proper Fluid to be employed on theſe occaſions.

It is remarkable, that *Tartar vitriolat. Sal Gem. Sal mirabile, Sal maritimum, Nitre, &c.* being Salts composed of different Acids and an Alkaline Salt, should so far exceed in gravity the Vitriolic Salts, composed of the most heavy Acid and a metallic Earth. Is not this owing to its forming less solid Chrystals, and to its containing large quantities of Air concealed in its Pores?

The great difference in the weight of the *Nitre*, in the several experiments of *Fahrenheit*, *Newton*, and *Freind*, may possibly be owing to the quantity of its concealed Air.

TAB. X.

Of Fluids.

MERCURY. *Ward. C.* (See Tab. I. among the Metals.)	14.000
Oleum Vitrioli. *Fahrenheit.* . .	1.8775*
Oyl of Vitriol. *Newton. C* .	1.700
Spiritus Nitri Hermeticus. *Freind.* .	1.760
Id. *Musschenb.* . . .	1.610
Lixivium cineris clavellati, sale quantum fieri potuit impregnatum. *Fahrenheit.*	1.5713*
Id. alio tempore præparatum. *Fahrenh.*	1.5634*
Oil of Tartar. *Cotes.* Ol. Tartari per deliquium. *Musschenb.* . . .	1.550
Spiritus Nitri, cum Ol. Vitrioli. *Freind.*	1.440
Id. *Musschenb.* . . .	1.338
Spiritus Nitri communis. Item, Bezoardicus. *Freind.* . . .	1.410

Spirit

Spirit of Nitre. *Cotes.* Item Sp. Nit. Bezoardicus. *Mufschenb.* . .	1.315
Sp. Nitri. *Fahrenheit.* . .	1.2935*
Sp. Nitri dulcis. *Mufschenb.* . .	1.000
Aqua fortis melioris notæ. *Fahrenheit.*	1.409*
Eadem, duplex. *Freind.* . .	1.340
Aqua fortis. *Cotes.* . . .	1.300
Eadem, fimplex. *Freind* . .	1.100
Solutio falis comm. in aqua faturata. *Davies.*	1.244
Eadem, 1 in aquæ 2,7 part. ponderis. *Davies.*	1.240
Eadem 1 in aquæ 3 part. *Davies.*	1.217
Eadem, 1 in aquæ 3 part. *Freind.* .	1.146
Eadem, 1 in aquæ 12 part. *Davies.*	1.060
Soap Lees the ftrongeft. *Jurin.* .	1.200
D°. Capital. *Jurin.* . . .	1.167
Spirit of Vitriol. *Freind.* . .	1.200
Spiritus Salis cum Ol. Vitriol. *Mufschenb.*	1.154
Idem, &c. *Freind.* . .	1.146
Spirit of Salt. *Cotes.* Sp. Salis marini. *Mufschenb.* . . .	1.130
Sp. Salis communis. *Freind.* . .	1.037
Sp. Salis dulcis. *Mufsch.* . .	0.951
Id. *Freind.*	0.890
Sp. Salis Ammoniaci fuccinat. Item, cum ciner. clavellat. *Freind.* . .	1.120
Sp. Salis Ammoniac. cum calce. *Mufsch.*	0.952
Idem cum calce viva. *Freind.* .	0.890
Sp. Cornu Cervi non rectific. *Freind.* .	1.073
Sp. Serici. *Mufschenb.* . . .	1.145
Sp. Urinæ. *Cotes.*	1.120
Solutio Salis enixi, 1 in aquæ 5 part. *Freind.*	1.100

Oleum Saſſafras. *Muſschenb.* . .	1.094
Decoctio Gentianæ. *Freind.* . .	1.080
Sp. Tartari. *Freind. Muſschenb.* .	1.073
Decoctio Biſtortæ. *Freind.* . .	1.073
Decoctio Sarzæ. It. Chinæ. *Freind.*	1.049
Decoctio Ari. It. Sp. Salis comm. *Freind.*	1.037
Oleum Cinnamomi. *Muſschenb.* . .	1.035
Ol. Caryophyllorum. *Muſschenb.* .	1.034
Beer Vinegar. *Oxf. Soc.* . .	1.034
Acetum Vini. *Muſschenb.* . .	1.011
Id. diſtillatum. *Muſschenb.* . .	0.994
Acetum. *Freind.* . . .	0.976
Sack. *Oxf. Soc.* . . .	1.033
Sp. Ambræ. *Muſschenb.* . . .	1.031
Sea-Water. *Cotes.* . . .	1.030
D°. ſettled clear. *Oxf. Soc. Ward.* .	1.027
College plain Ale. *Oxf. Soc.* . .	1.028
Solutio Aluminis, 1 in aquæ 5.33 part. Item Solutio Sal. Amm. purif. 1, et vitriol. alb. 1, in aquæ 5 part. *Freind.*	1.024
Laudanum liquidum Sydenhami. It. Panacea Opii. *Freind.*	1.024
Decoctio Cort. Peruv. Item, Granatorum. *Freind.*	1.024
Moil Cyder, not clear. *Oxf. Soc.* .	1.017
Aqua fluviatilis. *Muſschenb.* . .	1.009
Tinctura Aloes cum aqua. Item, Decoctio Santali rubri. *Freind.* . . .	1.000
Rain Water. *Newton, Reynolds.* Common Water. *Cotes.* Common clear Water. *Ward.* Pump Water. *Oxf. Soc. J. C.* Aqua. *Ghetaldus.* Aqua pluviatilis. *Fahrenheit, Muſschenb. &c.* . .	1.000

Aqua

Aqua vel Vinum. *Villalpandus.*	1.000
Aqua putealis. *Muſchenb.*	0.999
Oleum Fœniculi. *Muſchenb.*	0.997
Oleum Anethi. *Muſchenb.*	0.994
Aqua diſtillata. *Muſchenb.*	0.993
Wine, Claret. *Oxf. Soc.*	0.993
D°. red. *Ward.*	0.992
Vinum. *Petitus.*	0.984
Id. *Ghetaldus.* (ad aquam ut $98\frac{1}{3}$ ad 100.)	0.983
Id. Burgundicum. *Muſchenb.*	0.953
Oleum Sabinæ. It. Hyſſopi. *Muſchenb.*	0.986
Ol. Ambræ. It. Pulegii. *Muſchenb.*	0.978
Ol. Menthæ. It. Cumini. *Muſchenb.*	0.975
Decoctio Sabinæ. *Freind.*	0.960
Infuſio Marrhubii. It. Menthæ. It. Abſynth. *Freind.*	0.950
Ol. Nucis Moſchatæ. *Muſchenb.*	0.948
Ol. Tanaceti. *Muſchenb.*	0.946
Ol. Origani. It. Carvi. *Muſchenb.*	0.940
Elixir Propr. cum Sale volat. It. Infuſio Theæ. *Freind.*	0.940
Ol. Spicæ. *Muſchenb.*	0.936
Ol. Roriſmarini. *Muſchenb.*	0.934
Linſeed Oyl. *Newton. C.*	0.932
D°. *Ward.*	0.931
Spirits of Wine proof, or Brandy. *Ward.*	0.927
Sp. of Wine well rectified. *Newton. C.*	0.866
Alcohol Vini. *Fahrenheit.*	0.826
Id. magis dephlegmatum. *Fahrenheit.*	0.825
Sp. Vini. *Freind.*	0.818
Id. rectific. *Freind.*	0.781
Eſprit de Vin etherè. *Muſchenb.*	0.732

Spiritus

Spiritus Croci. *Freind.*	0.925
Lamp Oyl. *Reynolds.*	0.924
Oleum. *Ghetaldus.* (ad aquam ut 91$\frac{2}{3}$ ad 100.)	0.916
Oyl Olive. *Newton. C.*	0.913
D°. *Ward.*	0.912
Sallad Oyl. *Reynolds.*	0.904
Oleum. *Villalpandus.*	0.900
Id. *Petitus.*	0.891
Ol. Raparum. *Fahrenheit.*	0.913
Id. It. Tinct. Chalyb. Mynsicht. It. Tinct. Sulphur cum Sp. Terebynth. *Freind.* It. Huile de semences de navets. *Musch.*	0.853
Sp. Mellis. *Musschenb.*	0.895
Sp. Salis Ammoniaci cum calce viva.	0.890
Oleum Aurantiorum. *Muschenb.*	0.888
Spirit of Turpentine. *Newton. C.*	0.874
Tinct. Castorei. Item Sp. Vini camphorat. *Freind.*	0.870
Oyl of Turpentine. *Boyle* V. 22. *a.*	0.864
Ol. Terebynth. *Freind.*	0.793
Ol. Ceræ. *Musschenb.*	0.831
Tinctura Corallii. *Freind.*	0.828
Aqua cocta. *Freind.*	0.750
Air. *Newton. C.*	0.00125
Aer *Princip. Edit.* 3. *p.* 512. Aer juxta superficiem terræ occupat quasi spatium 850 partibus majus quam aqua ejusdem ponderis.	0.00118
The same, by an experiment made by the late Mr. Francis Hauksbee F.R.S. when the barometer stood at 29.7 inches. See *Physico Mathem. Exp. pag.* 74.	0.00113

As to the abſolute weight of water with which all the other bodies are compared in theſe Tables, Mr. *Boyle* tells us in his *Medicina Hydroſtatica*, printed in the new Edition of his Works, V. 19. *b*. that he had found by his own experiments, that a cubic inch of clear water weighed 256 *Troy* Grains. And Mr. *Ward* of *Cheſter*, who afterwards purſued this affair with great accuracy, determined that a cubic inch of common clear water did weigh by his tryals 253.18 like Troy Grains, or 0.527458 decimals of the *Troy Ounce*, or 0.578697 of the *Ounce Averdupois*, agreeable to what Mr. *Reynolds* had formerly deliver'd, who found the inch cubic of Rain Water to weigh by his experiments 0.579036 decimals of the ſame Averdupois ounce, differing from the other only 0.000339 parts.

But, as the accuracy of all the experiments in theſe tables depends upon the identity of the weight of Common Water, it may not be improper to aſcertain that point by a Note taken from Mr. *Boyle's Medicina Hydroſtatica*, V. 18. *b*. where he expreſſes himſelf in the following manner.

—— " It ſpeciouſly may, and probably will be " objected, that —— there may be a great diſparity " betwixt the liquors that are called, and that de- " ſervedly, *Common Water*. And ſome travellers " tell us from the preſs, that the water of a certain " eaſtern river, which if I miſtake not is *Ganges*, " is by a fifth part lighter than our water. But—— " having had upon ſeveral occaſions the opportunity " as well as curioſity to examine the weight of " divers waters, ſome of them taken up in places very

" diſtant

" diſtant from one another. I found the difference " between their ſpecific gravities far leſs than almoſt " any body would expect. And if I be not much " deceived by my memory (which I muſt have " recourſe to, becauſe I have not by me the notes " I took of thoſe trials) the difference between " waters, where one would expect a notable diſpa- " rity, was but about the thouſandth part (and " ſometimes perchance very far leſs) of the weight " of either. Nor did I find any difference con- " ſiderable in reference to our queſtion, between " the weight of divers waters of different kinds, as " ſpring-water, river-water, rain-water, and ſnow- " water; though this laſt was ſomewhat lighter " than any of the reſt. And having had the curio- " ſity to procure ſome water brought into *England*, " if I much miſremember not, from the river " *Ganges* itſelf; I found it very little, if at all, " lighter than ſome of our common waters."

The heavieſt fluid we are acquainted with, next to *Mercury*, is *Oyl* of *Vitriol*, or water impregnated with the *Vitriolic Acid* in the higheſt degree we can obtain it, being almoſt double the weight of Water.

The next is probably the *ſaturated ſolution* of the *fix'd Salt of Vegetables*; being a ponderous Salt, and diſſolving freely in Water.

The next to this is *Spirit of Nitre*. *Spirit of Salt* is lighter, and inferior in weight to the *ſaturated ſolution* of *Salt* itſelf.

It is obſervable, that *marine* or *common Salt* and *Nitre* differ little in gravity, contrary to the nature of their *Spirits*.

The

The ſeveral *ſolutions* of *common Salt*, if accurately repeated, would ſhew in what proportion the gravities of fluids increaſe, upon the addition of Salt: and that *Sea-Water* does not contain one twenty-fourth part of Salt.

I have omitted in this table the three animal fluids, Milk, Serum of Blood, and Urine, as the ſame may be ſeen before in the 8*th* table, that of *animal parts*; but it may be noted in general that the ſpecific gravity of all theſe fluids is nearly the ſame as that of Sea Water.

There are in Dr. *Freind's* table ſeveral decoctions of Plants, which I have inſerted, altho' they are not I think of much uſe, nor greatly to be depended upon. Several of them are lighter than common Water, in contradiction to Dr. *Jurin's* obſervation, that *Vegetable Parts* are all heavier than Water: But it is probable theſe Experiments were made before the *Decoctions* were reduced to the temper of *Common Water*.

What is meant by the *Aqua cocta* of Dr. *Freind* in his table, I cannot imagine; not having any idea of ſuch a change by boiling or otherwiſe, as can deprive common water of a full fourth part of its weight.

Since the denſity of the Air is as the force by which it is compreſſed, it follows that the weight of any portion of Air muſt vary in the ſame proportion with the weight of the whole *Atmoſphere*: which in our climate is not leſs than one tenth of the whole weight, allowing the *Barometer* to vary from 28 to 31 Inches.

Again, by an experiment of the late Mr. *Hauksbee's* in his *Phyſ. Mechan. exp.* pag. 170. the denſity of the air varies one eighth part between the greateſt degree of Heat in Summer, and that of Cold in the Winter Seaſon. So that the Air, in a hard froſt when the *Mercury* ſtands at 31 inches, is near a fifth part ſpecifically heavier, than it is in a hot day when the *Mercury* ſtands at 28 inches.

TAB. XI.

From Monſ. Homberg *and* John Caſpar Eiſenſchmid, *of the proportion of the ſpecific weights of certain fluids in the Winter to the weights of the ſame in the Summer Seaſon.*

Mercurius . . .	1.00479
Aqua pluvialis . . .	1.00809
Aqua fluviatilis . . .	1.00811
Aqua diſtillata . . .	1.00815
Spirit. Vitriol. . . .	1.01272
Lac bubulum . . .	1.01316
Aqua marina	1.01351
Spir. Salis . . .	1.01467
Acetum	1.01600
Ol. Vitrioli	1.02131
Ol. Terebynth. . . .	1.02141
Aqua fortis . . .	1.02637
Ol. Tartari	1.03013
Spir. Vini . . .	1.03125
Spir. Nitri	1,04386

The

The Oyls of Olive and ſweet Almonds congealing with the cold, could not be examin'd by the *Aræometer* in the winter ſeaſon.

According to this table, the increaſe of the ſpecific weight of common water in the winter above its weight in the ſummer, is not more than about the one hundred and twenty-fourth part of the whole; which is little more than half of what Profeſſor *Muſschenbroek* has elſewhere accounted the ſame, *deſorte qu' un pied cubique Rhenan d'Eau, qui peſe environ 64 livres en Etè, ſe trouvera etre en Hiver de preſque 65 livres. Eſſai de Phyſique p. 424.* but ſure this difference is much too great.

Notwithſtanding that all fluids are condenſed by cold, it is only till ſuch time as they are ready to freeze; for upon the freezing they immediately expand again, ſo as for the ice to be lighter ſpecifically than the fluid of which it is formed, and to ſwim in it: *Muſschenbroek* gives the ſpecific weight of Ice to be to that of Water commonly as 8 to 9. *La peſanteur de la Glace eſt ordinairement a celle de l'Eau, comme* 8 *a* 9. pag, 441. I am not acquainted with any other accurate experiments upon this ſubject, and it is hard to get ice in which there are not large bubbles of air included.

The *Philoſophical Society* at *Oxford*, together with their Table of *Specific Gravity* already ſo often mentioned in the foregoing pages, communicated beſides at the ſame time, to the *Royal Society*, another Table of a groſſer nature indeed, but which being printed in the ſame Number 169. of the *Philoſophical Tranſactions*, and appearing to be of uſe for many purpoſes: I have thought

 the

the ſame not improper to be here alſo tranſcribed.

Of the weight of a cubic foot of divers grains &c, tried in a veſſel of well-ſeaſon'd Oak, whoſe concave was an exact cubic foot.

The following bodies were poured gently into the veſſel, and thoſe in the firſt 12 experiments were weigh'd in ſcales turning with two ounces; but the laſt 7 were weighed in ſcales turning with one ounce. The pounds and ounces here mentioned are Averdupois weight.

	℔	℥
1. A foot of *Wheat* (worth 6 *s.* a buſhel).	47	8
2. *Wheat* of the beſt ſort (worth 6 *s.* 4 *d.* a buſhel). Both ſorts were red *Lammas Wheat* of laſt year. . . .	48	4
3. The ſame ſort of *Wheat* meaſured a ſecond time.	48	2
4. White *Oats* of the laſt year. . . The beſt ſort of *Oats* were 2 *d* in a buſhel better than theſe.	29	8
5. Blew *Peaſe* (of the laſt year) and much worm-eaten.	49	12
6. White *Peaſe* of the laſt year but one	50	8
7. *Barley* of the laſt year (the beſt ſort ſells for 1 *s.* 6 *d.* in a quarter more than this)	41	2
8. *Malt* of the laſt year's *Barley*, made 2 months before. . . .	30	4
9. Field *Beans* of the laſt year but one. .	50	8

10.

	℔	℥
10. Wheaten *Meal* (unſifted).	31	0
11. Rye *Meal* (unſifted).	28	4
12. Pump *Water*.	62	8
13. Bay *Salt*.	54	1
14. White Sea *Salt*.	43	12
15. *Sand*.	85	4
16. Newcaſtle *Coal*.	67	12
17. Pit *Coal*, from *Wednesbury* 63; but this is very uncertain in the filling the interſtices betwixt the greater pieces.	63	0
18. *Gravel*.	109	5
19. Wood *Aſhes*.	58	5

Of the ſame nature is alſo the following account of *The difference of the weight of ſome Liquors upon the Tunn compared to Rain Water*, from the Experiments made formerly by Mr. *Reynolds* in the *Tower* of *London*, and communicated to the *Royal Society*, with his others before-mentioned, by Mr. *Smethwick*, *July* 7. 1670.

	℔	℥ *Averdup.*
Muſcadine Wine was found heavier than Rain Water	11	2
Milk	8	4
Sherry	5	3
Ale	5	2
Canary Wine	3	3
Small Beer	1	3
White Wine was found lighter than Rain Water	1	2

Rheniſh

	lb	oz Averdup.
Rhenish Wine	1	4
Claret	1	6
Sallet Oyl	21	6

The proportion given by this Author as the true one of the *Averdupois* Pound to the *Troy* Pound is, that fourteen of the former are equal to seventeen of the latter.

From whence the *Averdupois* Pound would be found equal to 6994.285, and the *Ounce* to 437.143 *Troy Grains*; which is indeed a little less than the same have since been determined by others; for Mr. *Ward* of *Chester* gives from a very nice experiment as he calls it, of his own, that one pound Averdupois was equal to 14 ounces 11 penyweight and $15\frac{1}{2}$ *Troy Grains*, or to $6999\frac{1}{2}$, and consequently the ounce Averdupois to 437.47 of the same grains. And several Gentlemen of the *Royal Society*, who very carefully on 22 *April* 1743. examined the original standards of weights kept in the *Chamberlain's Office* of his MAJESTY'S *Exchequer*, found, upon the medium of the several trials which they made with those standards, that the *Pound Averdupois* was equal to 7000.14, and the *Ounce Averdupois* to 437.51 *Troy Grains*. *Phil. Trans* N°. 470.

I shall conclude these papers with the two Tables from *Marinus Ghetaldus* mentioned in the beginning, which I here transcribe, with an account of some of their uses, in his own words.

4

Ad comparandum inter se duodecim corporum genera, gravitate, et magnitudine Tabella.

	Aurum	Arg. vivum	Plumb.	Argent.	Æs	Ferrum	Stann.	Mel	Aqua	Vinum	Cera	Oleum
Oleum	$20\frac{8}{11}$	$14\frac{62}{77}$	$12\frac{6}{11}$	$11\frac{3}{11}$	$9\frac{9}{11}$	$8\frac{8}{11}$	$8\frac{4}{55}$	$1\frac{32}{55}$	$1\frac{1}{11}$	$1\frac{4}{55}$	$1\frac{5}{121}$	1
Cera	$19\frac{19}{21}$	$14\frac{32}{147}$	$12\frac{1}{21}$	$10\frac{52}{63}$	$9\frac{9}{21}$	$8\frac{8}{21}$	$7\frac{89}{105}$	$1\frac{109}{210}$	$1\frac{1}{21}$	$1\frac{13}{420}$	1	
Vinum	$19\frac{19}{59}$	$13\frac{331}{413}$	$11\frac{41}{59}$	$10\frac{30}{59}$	$9\frac{9}{59}$	$8\frac{8}{59}$	$7\frac{31}{59}$	$1\frac{28}{59}$	$1\frac{1}{59}$	1		
Aqua	19	$13\frac{4}{7}$	$11\frac{1}{2}$	$10\frac{1}{3}$	9	8	$7\frac{2}{5}$	$1\frac{9}{20}$	1			
Mel	$13\frac{3}{29}$	$9\frac{73}{203}$	$7\frac{27}{29}$	$7\frac{11}{87}$	$6\frac{6}{29}$	$5\frac{15}{29}$	$5\frac{3}{29}$	1				
Stannum	$2\frac{21}{37}$	$1\frac{221}{259}$	$1\frac{41}{74}$	$1\frac{44}{111}$	$1\frac{8}{37}$	$1\frac{3}{37}$	1					
Ferrum	$2\frac{3}{8}$	$1\frac{39}{56}$	$1\frac{7}{16}$	$1\frac{7}{24}$	$1\frac{1}{8}$	1						
Æs	$2\frac{1}{9}$	$1\frac{22}{63}$	$1\frac{5}{18}$	$1\frac{4}{27}$	1							
Argentum	$1\frac{26}{31}$	$1\frac{68}{217}$	$1\frac{7}{62}$	1								
Plumbum	$1\frac{15}{23}$	$1\frac{29}{161}$	1									
Arg. viv.	$1\frac{38}{95}$	1										
Aurum	1											

Quæro,

Quæro, exempli gratia, quam habet rationem in gravitate plumbum ad aurum. Intelligatur plumbum, quoniam levius est auro, gravitatem habere 1, *et in linea plumbi, in prima columna nominata, sub titulo auri, quæratur auri gravitas, ea erit* $1\frac{15}{23}$. *Plumbum igitur ad aurum rationem habebit in gravitate ut* 1, *ad* $1\frac{15}{23}$. *Si enim sumantur duo corpora magnitudine æqualia, unum plumbeum alterum aureum, sit autem plumbei corporis gravitas* 1, *aurei erit* $1\frac{15}{23}$; *quare corpus plumbeum ad corpus aureum ejusdem magnitudinis rationem habebit in gravitate ut* 1, *ad* $1\frac{15}{23}$. *Comparantur autem inter se genera diversa gravitate, in corporibus magnitudine æqualibus.*

Rursus, quæro quam habet rationem in gravitate aqua ad argentum vivum. Intelligatur aqua, ut levior argento vivo gravitatem habere 1, *et in linea aquæ, sub titulo argenti vivi, quæratur argenti vivi gravitas, ea erit* $13\frac{4}{7}$; *aqua igitur ad argentum vivum rationem habebit in gravitate ut* 1, *ad* $13\frac{4}{7}$.

Contra, quæro quomodo se habent in magnitudine aurum et plumbum. Intelligatur aurum, quoniam gravius est plumbo, magnitudinem habere 1, *et in linea plumbi, sub titulo auri, quæratur plumbi magnitudo, ea erit* $1\frac{15}{23}$; *aurum igitur ad plumbum se habebit in magnitudine ut* 1, *ad* $1\frac{15}{23}$: *si enim sumantur duo corpora æque gravia, unum aureum, alterum plumbeum, sit autem corporis aurei magnitudo* 1, *plumbei erit* $1\frac{15}{23}$; *quare corpus aureum ad corpus plumbeum ejusdem gravitatis se habebit in magnitudine ut* 1, *ad* $1\frac{15}{23}$. *Comparantur autem inter se genera diversa magnitudine, in corporibus æque gravibus.*

Quæro

Quæro denique, quomodo se habent in magnitudine ferrum, et aqua, ponatur ferrum, ut gravius aqua, magnitudinem habere 1, *et in linea aquæ, sub titulo ferri, quæratur aquæ magnitudo, ea erit* 8, *ferrum igitur ad aquam se habebit in magnitudine ut* 1, *ad* 8.

Altera,

Altera, ad comparandum inter se duodecim corporum genera, gravitate, et magnitudine, Tabella.

	Aurum.	Arg. viv.	Plum.	Argent.	Æs	Ferrum	Stann.	Mel	Aqua	Vinum	Cera	Oleum
Aurum	100	$71\frac{3}{7}$	$60\frac{10}{19}$	$54\frac{22}{57}$	$47\frac{7}{19}$	$42\frac{2}{19}$	$38\frac{18}{69}$	$7\frac{12}{190}$	$5\frac{5}{19}$	$5\frac{10}{57}$	$5\frac{5}{209}$	$4\frac{47}{57}$
Arg. viv.		100	$84\frac{14}{19}$	$76\frac{8}{57}$	$66\frac{6}{19}$	$58\frac{18}{19}$	$54\frac{10}{19}$	$10\frac{13}{19}$	$7\frac{7}{19}$	$7\frac{14}{57}$	$7\frac{7}{209}$	$6\frac{43}{57}$
Plumbum			100	$89\frac{59}{69}$	$78\frac{6}{23}$	$69\frac{13}{23}$	$64\frac{8}{23}$	$12\frac{19}{23}$	$8\frac{16}{23}$	$8\frac{38}{69}$	$8\frac{76}{253}$	$7\frac{67}{69}$
Argent.				100	$87\frac{3}{31}$	$77\frac{13}{31}$	$71\frac{19}{31}$	$14\frac{1}{31}$	$9\frac{21}{31}$	$9\frac{16}{31}$	$9\frac{81}{341}$	$8\frac{27}{31}$
Æs					100	$88\frac{8}{9}$	$82\frac{2}{9}$	$16\frac{1}{9}$	$11\frac{1}{9}$	$10\frac{25}{27}$	$10\frac{20}{33}$	$10\frac{5}{27}$
Ferrum						100	$92\frac{1}{2}$	$18\frac{1}{8}$	$12\frac{1}{2}$	$12\frac{7}{24}$	$11\frac{41}{44}$	$11\frac{11}{24}$
Stannum							100	$19\frac{27}{37}$	$13\frac{19}{37}$	$13\frac{32}{111}$	$12\frac{366}{407}$	$12\frac{43}{111}$
Mel								100	$68\frac{28}{29}$	$67\frac{71}{87}$	$65\frac{265}{319}$	$63\frac{19}{87}$
Aqua									100	$98\frac{1}{3}$	$95\frac{5}{11}$	$91\frac{2}{3}$
Vinum										100	$97\frac{47}{649}$	$93\frac{13}{59}$
Cera											100	$96\frac{2}{63}$
Oleum												100

Quæro exempli gratia, quænam ſit ratio in gravitate, auri ad argentum. Intelligatur aurum quoniam gravius eſt argento, gravitatem habere 100, *et in linea auri, ſub titulo argenti, reperietur argenti gravitas* $54\frac{22}{57}$, *aurum igitur ad argentum rationem habebit in gravitate ut* 100, *ad* $54\frac{22}{57}$. *Si enim ſumantur duo corpora, magnitudine æqualia, unum aureum, alterum argenteum, ſit autem aurei corporis gravitas* 100, *erit argentei* $54\frac{22}{57}$; *quare corpus aureum ad corpus argenteum ejuſdem magnitudinis, rationem habebit in gravitate, ut* 100, *ad* $54\frac{22}{57}$.

Quæro, quomodo ſe habet in gravitate aqua ad vinum; quoniam aqua gravior eſt vino, intelligatur ejus gravitas 100, *et quoniam in linea aquæ, ſub titulo vini, datur vini gravitas* $98\frac{1}{3}$, *aqua ad vinum ſe habebit in gravitate, ut* 100, *ad* $98\frac{1}{3}$.

Contra quæro quomodo ſe habent in magnitudine argentum, et aurum. Intelligatur argentum ut levius auro, magnitudinem habere 100, *et in linea auri, ſub titulo argenti, quæratur auri magnitudo, ea erit* $54\frac{22}{57}$, *argentum igitur ad aurum ſe habebit in magnitudine, ut* 100, *ad* $54\frac{22}{57}$. *Si enim ſumantur duo corpora æque gravia, unum argenteum, alterum aureum, ſit autem argentei corporis magnitudo* 100, *erit aurei* $54\frac{22}{57}$; *quare corpus argenteum, ad corpus aureum ejuſdem gravitatis, ſe habebit in magnitudine, ut* 100, *ad* $54\frac{22}{57}$.

Quæro denique, quomodo ſe habent in magnitudine aqua et argentum vivum. Quoniam aqua levior eſt argento vivo, intelligatur ejus magnitudo 100, *et in linea argenti vivi, ſub titulo aquæ, quæratur argenti vivi magnitudo, et reperietur* $7\frac{7}{19}$, *aqua igitur ad argentum vivum ſe habebit in magnitudine, ut* 100, *ad* $7\frac{7}{19}$.

FINIS.

ERRATA:

N°. 487. *in the Contents, Art.* VII. *and p.* 320, *Tit.* VII. The Rev. Charles Lyttleton LLD. F. R. S. *and for* Archdeacon of Exeter, *read* Dean of Exeter.

Printed for C. DAVIS, over-againſt *Gray's Inn Gate* in *Holbourn*, PRINTER to the ROYAL SOCIETY, M.DCC.XLIX.

Numb. 489.

PHILOSOPHICAL TRANSACTIONS.

For the Months of *October* and *November*, 1748.

The CONTENTS.

Printed for C. DAVIS, over-againſt *Gray's Inn Gate* in *Holbourn*, PRINTER to the ROYAL SOCIETY, M.DCC.XLIX.

I. *An Account of the Experiments made by some Gentlemen of the* Royal Society, *in order to measure the absolute Velocity of* Electricity; *communicated to the* Royal Society *by Mr.* W. Watson *F.R.S.*

Read Oct. 27. 1748.

I LAID before the *Royal Society* the Beginning of last Winter an Account * of what had been done by some Gentlemen, in order to ascertain the respective Velocities of Electricity and Sound; from which it appeared, that through a Space measuring 6732 Feet, the Electricity was perceptible in a Quantity of Time less than $\frac{837}{1000}$ of a Second. But the Gentlemen concerned were desirous, if possible, of ascertaining the absolute Velocity of Electricity at a certain Distance; and a Method had been thought of, by which this might be determined with great Exactness.

Accordingly, *August* 5. 1748. there met at *Shooter's Hill* for this Purpose, the *President* of the *Royal Society*, the Rev. Mr. *Birch*, the Rev. Dr. *Bradley*, Astronomer Royal, *James Burrow* Esq; Mr. *Ellicot*, Mr. *George Graham*, *Richard Graham* Esq; the Rev. Mr. *Lawrie*, *Charles Stanhope* Esq; and myself, who were of the *Royal Society*, Dr. *Bevis*, and Mr. *Grischow* jun. a Member of the *Royal Academy of Sciences* at *Berlin*.

It was agreed to make the electrical Circuit of two Miles; in the middle of which an Observer was to take in each Hand one of the Extremities of

* See these *Transf.* N°. 485.

a Wire, which was a Mile in Length. These Wires were to be so disposed, that this Observer being placed upon the Floor of the Room near the electrical Machine; the other Observers might be able in the same View to see the Explosion of the charged Phial, and the Observer holding the Wires; and might take notice of the Time lapsed between the discharging the Phial and the convulsive Motions of the Arms of the Observer in consequence thereof; inasmuch as this Time would shew the Velocity of Electricity, through a Space equal to the Length of the Wire between the coated Phial and this Observer.

The electrifying Machine was placed in the same House as it was last Year. We then found ourselves greatly embarassed by the Wire's being conducted by the Side of the Road, which we were compell'd to, on account of the Space necessary for the measuring of Sound: But so great a Distance from the Machine was not now wanted, though the Circuit through the Wire was intended to be at least two Miles. We had discover'd, by our former Experiments, that the only Caution now necessary was, that the Wires conducted upon dry Sticks should not touch the Ground, each other, or any Non-electric, in a considerable Degree, in any Part of their Length: If they did not touch each other, the Returns of the Wire, be they ever so frequent, imported little, as the Wire had been found to conduct Electricity so much better than the Sticks. It was therefore thought proper to place these Sticks in a Field fifty Yards distant from the Machine. The Length of this Field being eleven Chains, or 726 Feet, eight Returns of the Wire from the Top to

to the Bottom of the Field, made somewhat more than a Mile, and sixteen Returns more than two Miles, the Quantity of Wire intended for the Electricity to pass through to make the Experiment.

We had found last Year, that, upon discharging the electrified Phials, if two Observers made their Bodies Part of the Circuit, one of which grasped the leaden Coating of the Phial in one Hand, and held in his other one Extremity of the conducting Wire; and if the other Observer held the other Extremity of the conducting Wire in one Hand, and took in his other the short iron Rod with which the Explosion was made; upon this Explosion, I say, they were both shocked in the same Instant, which was that of the Explosion of the Phial. If therefore an Observer, making his Body Part of the Circuit, was shocked in the Instant of the Explosion of the charged Phial in the middle of the Wire, no Doubt would remain of the Velocity of Electricity being instantaneous through the Length of that whole Wire. But if, on the contrary, the Time between making the Explosion, and seeing the Convulsions in the Arms of the Observer holding the conducting Wires, was great enough to be measured, we then should be able to ascertain its Velocity to the Distance equal to half the Quantity of Wire employed only, let the Manner of the Electricity's discharging itself be what it would.

It has been a Question with some, who have consider'd this Subject, whether the Electricity, in compleating the Circuit from the Matter contained in the Glass, passed either by the Wire in the Mouth to the Coating of the Glass, the contrary Way by

the Coating to the Wire in the Mouth, or otherwise directed itself both Ways at once? That the Electricity must pass off one of these three Ways, was certain, as the Explosion would not be complete, unless in the Instant thereof some Matter very non-electric communicated between the Wire in the Mouth, and the Coating of the Glass. Unless therefore the Observer was placed in the Centre of the conducting Wires, it might be objected, that the Experiment was not made with the Exactness necessary; because any Person, who was of Opinion that the Electricity directed itself from the Mouth of the Glass to the Coating, might object, if the Wire from the short iron Rod to the Observer was only half the Length of that between the Observer and the Coating of the Glass, that the Electricity, in the Time found, passed only through the short Wire, and *vice versa*. But if, as it was here thought proper, the Observer was placed in the Centre of the conducting Wire, let the Direction of the Electricity be what it would, no Difference could happen in the Result of the Experiments, if made with the necessary Caution; because, if the Effects in the middle and both Ends of the Wires were instantaneous, the Conclusion therefrom would be very obvious. To make the Experiment, the same Phial filled with Filings of Iron, and coated with Sheet-Lead, which was used last Year, was placed in the Window of the Room near the Machine, and was connected to the prime Conductor by a Piece of Wire. To the Coating of this Phial a Wire was fastened; which, being conducted upon dry Sticks to the before-mentioned Field, was carried in like manner to the Bottom;

tom; and being conducted thus from the Bottom of the Field to the Top, and from the Top to the Bottom feven other times, returned again into the Room and was held in one Hand of an Obferver near the Machine. From the other Hand of this Obferver, another Wire, of the fame Length with the former, was conducted in the fame manner, and returned into the Room, and was faften'd to the iron Rod with which the Explofion was made. The whole Length of the Wires, allowing ten Yards for their Turns round the Sticks, amounted to two Miles a Quarter and fix Chains, or 12276 Feet.

As the Night preceding thefe Experiments had been very rainy, Care was taken, by filk Lines properly difpofed, that the Wires in their Paffage from the Window of the Houfe might not touch the Wood thereof; left, from the Moifture of this Wood, the electrical Circuit might be fhortened.

When all Parts of the Apparatus were properly difpofed, feveral Explofions of the charged Phial were made; and it was invariably feen, that the Obferver holding in each Hand one of the Extremities of thefe Wires was convulfed in both his Arms in the Inftant of making the Explofions.

Inftead of one, four Men were then placed holding each other by the Hand near the Machine, the firft of which held in his right Hand one Extremity of the Wire, and the laft Man the other in his left. They were all feen convulfed in the Inftant of the Explofion. Every one who felt it, complained of the Severity of the Shock.

It was then defired, by one of the Gentlemen concerned, that an Explofion fhould be made with the Obferver holding only one of the Wires. This

was

was done accordingly; but the Obſervér felt nothing, the Phial diſcharging itſelf in a different manner to what it did before, on account of the Circuit's not being compleated.

It was then tried, whether an Obſerver would be ſhocked upon the Diſcharge of the Phial, if the two Wires at their Extremities ſlightly touched each other, whilſt an Obſerver at the ſame time held one of theſe about a Foot from their Ends in each of his Hands. Upon Trial he felt nothing, though the Phial exploded very quick, becauſe the iron Wire conducted the Electricity better than the Body of the Obſerver.

It was then tried, whether or no, as the Ground was wet, if the Exploſion was made with the Obſerver holding the Extremity of each Wire ſtanding upon the Ground near the Window of the Houſe, any Difference would ariſe in the Succeſs of the Experiment. No Difference was found, the Obſerver being ſhocked in the Inſtant of the Exploſion, as before, in both his Arms, and acroſs his Breaſt.

Upon theſe Conſiderations we were fully ſatiſfied, that through the whole Length of this Wire, being, as I mentioned before, twelve thouſand two hundred and ſeventy-ſix Feet, the Velocity of Electricity was inſtantaneous.

As it was found laſt Year, we obſerved again, that although the electrical Commotions were very ſevere to thoſe who held the Wires, the Report of the Exploſion at the prime Conductor was little, in compariſon of that which is heard when the Circuit is ſhort. From whence it was conjectured, that the very loud Report, in the Experiment of *Leyden* is confined to a very ſhort Circuit.

II. *An Account of* double Fœtus's *of* Calves, *by Monſ* le Cat, *M.D. F.R.S.* &c. *dated at* Rouen, Auguſt 20. 1748. N. S. *Tranſlated from the* French *by* T. S. *M. D. F. R. S.*

Read Oct. 27. 1748.

I HAVE, ſince the Month of *January* 1735. been in Poſſeſſion of a Child, born in our City of *Rouen*, which has two Heads, four Arms, four lower Extremities, and two Trunks united, and as it were blended together. About that time I publiſhed * a Deſcription of the internal Parts of this Monſter, which had but one Heart; but I did not cauſe Draughts to be taken of thoſe Parts: and it would now be a difficult Matter to have them drawn ſo as to exhibit a good Repreſentation of the State in which they then were. This Negligence, through which I am deprived of thoſe curious and inſtructive Figures, which this monſtrous Birth would have afforded, made me wiſh for a like Opportunity, in ſome meaſure at leaſt to make amends for that Fault. This Opportunity preſented itſelf in *January* 1748, not in a human *Fœtus*, but in a Calf, which the Butchers of our Hoſpital cut out of a Cow.

The Deſcription which I ſhall give of this Monſter, will be the Explanation of the Figures that repreſent it.

Plate

* *Journal de Verdun*, for *March* 1735. p. 194.

TAB. I. *Fig.* 1.

The outward Surface of the double Calf is here exhibited, about one fourth of the natural Size.

Fig. 2.

The Integuments of the Breaſt being raiſed, there appears the Union and reciprocal Inſertion of the pectoral Muſcles of each Subject into one common *Linea alba.* None but the inmoſt Plans were attached to the Bones.

Fig. 3.

The Muſcles being removed, one *Sternum,* common to both Subjects, appears in Sight.

There was a *Sternum* intirely ſimilar to this, on the other or oppoſite Side.

TAB. II. *Fig.* 4.

A. The *Apex* of the Heart common to both.
B. The right Auricle of the Subject *B.*
C. The inferior *Aorta.*
D. The ſuperior *Aorta* of the Subject *B,* from which iſſue the right Subclavian †, and the Carotids **.
E. The pulmonary Artery of the ſame Subject *B.*
P, Its Lungs.
FF, The ſuperior *Venæ cavæ* of both Subjects.
ff, Their inferior *Venæ cavæ.*
GG. The *Thymus,* or Throat Sweet-Breads.
α, of Subject *A,* A Trunk formed by the Reunion of the Carotids **, and the Subclavians †; which Trunk commonly conſtitutes the ſuperior *Aorta,*

as


Fig. 1.
p. 498.

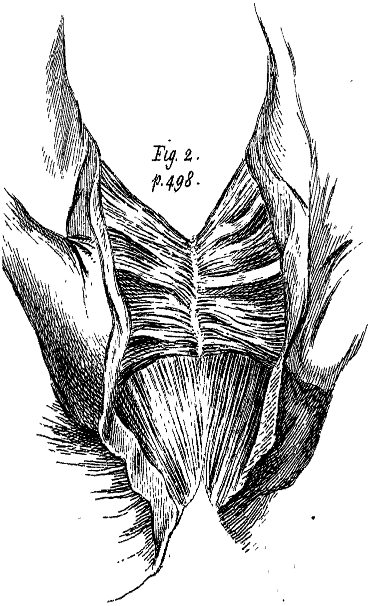

Fig. 2. p. 498.

The peice of Lath. p. 521.

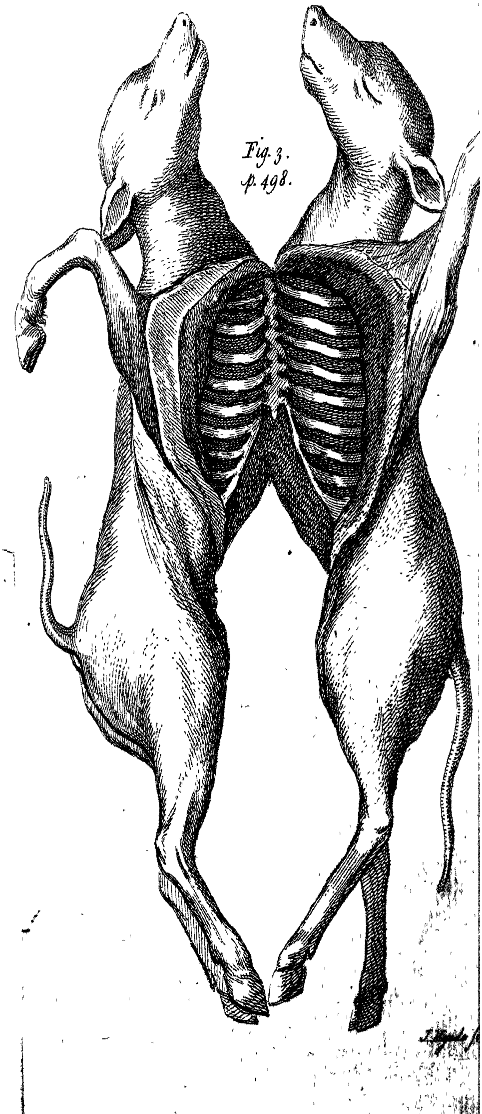

Fig. 3. p. 498.

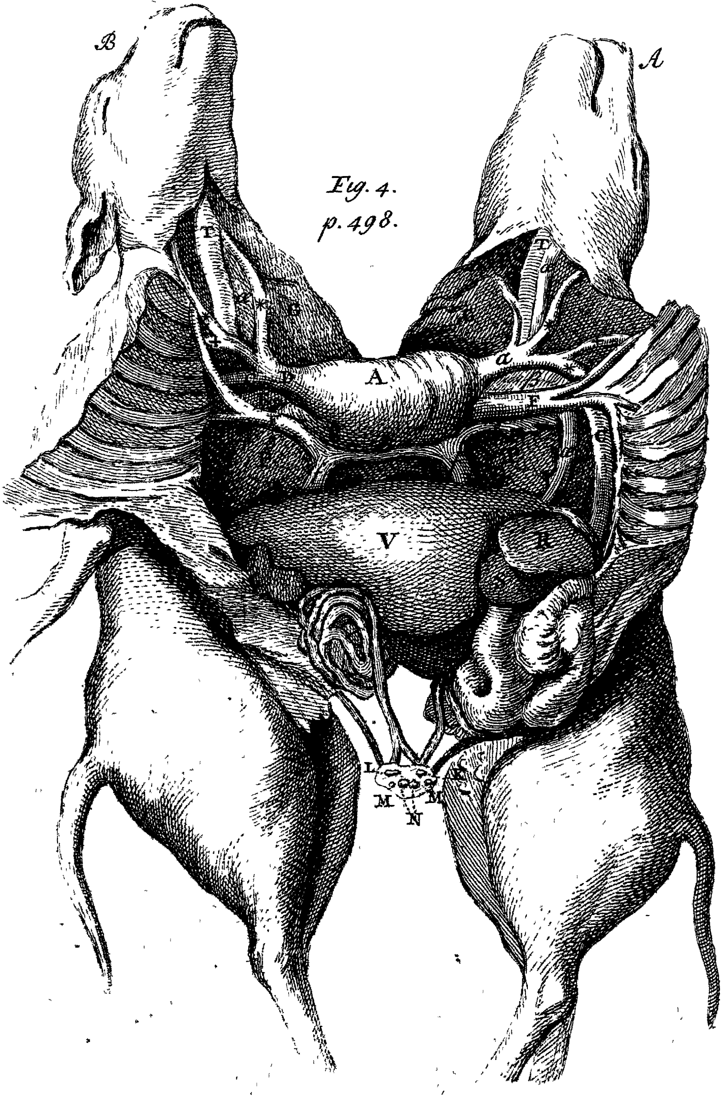

Fig. 4.
p. 498.

Fig. 5. p. 500.

J. Mynde sc.

as appears in the Subject *B:* but in this Subject *A*, it only ſends a ſmall *Canalis arterioſus*, γ, into the inferior *Aorta C*.

β, A thick common Trunk of the pulmonary Artery and the inferior *Aorta*. The latter plainly appears a Continuation of this Trunk; whereas it is commonly a Continuation of the *Aorta*, α, *D:* and the pulmonary Artery, β, *E*, only furniſhes the *Aorta*, α, γ, *C*, which makes but one Canal in ordinary Subjects, with a *Canalis arterioſus*, or Canal of Communication (See *Fig*. 5. in the Subject *B*). And indeed I am of Opinion, that this Structure, which ſeems extraordinary, is natural to every *Fœtus* that is not far advanced, as I explain it in my Courſe of Phyſiology under the Article of the *Fœtus*; and that it is a Conſequence and Proof of the mechanical and ſucceſſive Formation of the Organs of its Circulation, which begins by the lower Circle made by the umbilical Vein, as the firſt Mover; the Trunk of the *Vena cava*, the inferior *Aorta*, and the Branches of the *Vena cava*, which correſpond with it. Now the Subject *A* had ſeveral Marks, which demonſtrated that its Formation was leſs advanced than that of the Subject *B*.

æ,æ,æ, The *Oeſophagus* of each Subject. *TT*, The Windpipe.

R, The Spleen, *S*, the Stomach of the Subject *A*.

V, The Liver, which ſeems to belong to the Subject *B*.

Under them the great and ſmall Inteſtines.

K, The umbilical Vein of the Subject *A*, through which having made an Injection, the whole Liver

was injected, but the Heart and Veſſels were not: ſo that it is probable this Vein was diſtributed no farther than the Liver, and had no *Ductus venoſus* that paſs'd to the *Cava* and Heart.

L, The umbilical Vein of the Subject *B*, which received a large Branch of the umbilical Vein of the Subject *A*: and which Branch ſeemed to ſupply the Place of the venal Duct, that was wanting.

Having thrown in the Injection through this Vein *L*, the Heart and Veſſels of the two Subjects were injected.

MM, The Orifices of the umbilical Arteries, which were but two in Number, one for each Subject; the one and the other iſſuing from the right Iliac of each Subject.

N, The Openings of the *Urachi*, which were very large.

TAB. II. *Fig. 5.*

The other Side or View of this Monſter, wherein the Subject *A* is to the right; the Subject *B* to the left.

a, The *Thymus* of each Subject.
b, The right Auricle of the Subject *A*.
c, The left Auricle of the Subject *B*.
d, The ſuperior *Aorta* of the ſame.
e, Its pulmonary Artery.
f, The *Canalis arterioſus*, which here had the ſame Structure that all *Fœtus's* of nine Months or more uſually have.
g, Its inferior *Aorta*.
hh, The inferior *Venæ cavæ* of each Subject.

i, The

i, The *Azygos* of the Subject *B*.
k, Its *Oesophagus*.
l, Its Stomach.
m, Its Spleen.
nn, Part of the Lungs of each Subject.
o, The Liver of the Subject *A*.

TAB. II. *Fig*. 6.

This Plate repreſents the Heart, which is common to the two Subjects, laid open tranſverſally. The anterior Part is lifted up, to ſhew the Cavities and the Orifices of the Veſſels of its Baſe. The Figure is of about half the natural Size.

This Heart had only two Cavities, *D*, *E*, as uſual; but the right Cavity or Ventricle *D* belonged to the Subject *B*; and the left Ventricle *E* to the Subject *A*.

Into each of the Cavities *D E* there opened four Orifices; *viz.* two arterial, which were thoſe of the pulmonary Arteries, *a*, *a*, and of the *Aorta's*, *b*, *b*; and two venal Orifices, or thoſe of the right and left Auricles, for the Blood of the *Cavæ*, *c*, *c*, and of the pulmonary Veins, *d*, *d*.

I give the Name of *Aorta* to the ſuperior arterial Trunk *b*, of the Subject *A*, in Conformity with the uſual Appellations, and becauſe in common Subjects this Trunk alone deſerves that Name; although in this Caſe the pulmonary Artery *a* viſibly conſtitutes the principal Part of the inferior *Aorta*, *c*.

ff, The inferior *Cavæ*.

gg. The ſuperior *Cavæ*.

h, Part of the Branches of the pulmonary Artery.

k, The Valve of the Orifice of the right Auricle in the Subject *B*.

III. *Extract of a Letter from the Rev. Dr.* Doddridge *to* Henry Baker *F. R. S. concerning a* Wether *giving Suck to a* Lamb; *and of a* monstrous Lamb.

Dear Sir, *Northampton, July 2. 1748.*

Read Oct. 27. 1748.

THE Occasion of my writing to you now is to inform you of a remarkable Fact, which I have just heard from a Member of the Church of which I am Pastor, and in whom I can intirely confide. —— He tells me, that he has in *Upper Heyford Field*, about four Miles from this Town, a Wether-Sheep which now suckles a Lamb. I know not by what Accident, the Lamb sometime since ran after it, and fixed upon its Paps; drawing hard, Milk followed. The Lamb has subsisted very well upon what it sucked from him, and at the late shearing Time he himself pressed the Teats, and Milk came out in a considerable Quantity.

This reminds me of what Mr. *Ray* tells us from *Boccone*, that a Countryman in *Umbria* nourished his Child by his own Milk, and *Florentinus* and *Malpighius* are quoted on the same Occasion. *Bartholinus*, in his *Anatomy*, p. 215. has some remarkable Passages to this Purpose: he quotes a Passage in *Aristotle* concerning a He-Goat in *Lemnos*, who had a great Quantity of Milk.

I shall add to this a short Account of a monstrous Lamb, which was weaned in a Field near *Newport Pagnel* about the Middle of

last

laſt *March*, and was brought to me ſoon after it died. It had two perfect Heads (and two long Necks), each as large as that of a common Lamb, but ſucked only with that on the right Side. So far as I could learn the Organs of both were compleat. It walked only on four Legs, but had a fifth hanging down between the two Necks, rather longer than the other four; the Bones and Hoof were double, and had four Claws: the concave Side of it was turned upwards, and whenever the Creature walked this Leg moved up and down as it ſeemed ſpontaneouſly, and in a manner anſwerable to the Motion of the other four: it had two Tails, but no Vent behind: it had alſo two diſtinct Spines, but they met about five Inches above the Tail, and then divided again; but where they met they were not as one intire Spine, but as two adhering to each other. There were two Sets of Ribs, only thoſe which met upward (where the Spine ſhould regularly have been placed) were rather ſhorter than the other: and it ſeemed that the Blade-Bone belonging to the doubled Leg that grew between the Necks was larger than the reſt, and ſeemed to be two Bones, but not intirely diſtinct: it had two Hearts of equal Bigneſs, lying over each other almoſt like a *St. Andrew*'s Croſs, or (as we ſhould ſay in Heraldry) Saltire-wiſe. There were two *Oeſophagi* and two *Aſperæ Arteriæ*: four ſmall Lobes of Lungs, but the two Gullets were inſerted into one common Stomach. —— I am not Maſter enough of the Formation of ruminating Animals to inform you farther what was peculiar in this. We found nothing preternatural in the Formation of the Inteſtines, but the

the Tails grew so near, that the Return of both seemed to point to one Vent, tho' as I observed, the *Anus* was deficient. It had three Kidneys, one of them very large in proportion to the other two; so that I apprehend there was a Conjunction.

I send you this Account whilst the Matter is fresh in my Memory, and am,

Dear Sir,

Yours, &c.

P. Doddridge.

IV. *Abstract from a Letter sent by Monsieur* Buffon, *Member of the* Royal Academy *of* Sciences *at* Paris, *&c. to* Martin Folkes *Esq; Pr. R. S. concerning his Re-invention of* Archimedes's Burning Specula.

Read Oct. 27. 1748.

AS what I read some time since to our *Royal Academy* upon the Subject of my Re-invention of *Archimedes's* * burning *Specula*, cannot appear in our Memoirs before the Year 1747, I think of publishing by themselves my Observations upon these Mirrors, as soon as I shall satisfy myself upon certain Particulars, by some new Experiments I am now preparing to make. The *Speculum* I have already constructed, and which is but six Feet broad and as many high, burns Wood at the Distance of 200 Feet, it melts Tin and Lead at

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* See these *Trans.* No. 483.

at the Diſtance of above 120 Feet, and Silver at 50. The Theory which led me to this Diſcovery is founded upon two important Remarks, the one that the Heat is not proportional to the Quantity of Light, and the other that the Rays do not come parallel from the Sun. The firſt of thoſe, which appears to be a Paradox, is nevertheleſs a Truth, of which one may eaſily ſatisfy one's ſelf, by reflecting that Heat propagates itſelf even within Bodies; and that when one heats at the ſame time a large Superficies, the Firing is much quicker than when one only heats a ſmall Portion of the ſame.

I am, &c.

From the Chateau de Montbard *in* Burgundy, Sept. 18. N.S. 1748.

Buffon.

V. *An Eſſay on* Quantity; *occaſioned by reading a Treatiſe, in which* Simple *and* Compound Ratio's *are applied to* Virtue *and* Merit, *by the Rev. Mr.* Reid; *communicated in a Letter from the Rev.* Henry Miles *D.D. & F.R.S. to* Martin Folkes *Eſq; Pr.R.S.*

SECT. I.

What Quantity is.

Read Nov. 3. 1748. SINCE mathematical Demonſtration is thought to carry a peculiar Evidence along with it, which leaves no Room for further Diſpute; it may be of ſome Uſe, or Entertainment

tainment at leaſt, to inquire to what Subjects this kind of Proof may be applied.

Mathematics contain properly the Doctrine of Meaſure; and the Object of this Science is commonly ſaid to be Quantity; therefore Quantity ought to be defined, What may be meaſured. Thoſe who have defined Quantity to be whatever is capable of More or Leſs, have given too wide a Notion of it, which I apprehend has led ſome Perſons to apply mathematical Reaſoning to Subjects that do not admit of it.

Pain and Pleaſure admit of various Degrees, but who can pretend to meaſure them? Had this been poſſible, it is not to be doubted but we ſhould have had as diſtinct Names for their various Degrees, as we have for Meaſures of Length or Capacity; and a Patient ſhould have been able to deſcribe the Quantity of his Pain, as well as the Time it began, or the Part it affected. To talk intelligibly of the Quantity of Pain, we ſhould have ſome Standard to meaſure it by; ſome known Degree of it ſo well aſcertained, that all Men, when they talked of it, ſhould mean the ſame thing; we ſhould alſo be able to compare other Degrees of Pain with this, ſo as to perceive diſtinctly, not only whether they exceed or fall ſhort of it, but how far, or in what proportion; whether by an half, a fifth, or a tenth.

Whatever has Quantity, or is meaſurable, muſt be made up of Parts, which bear Proportion to one another, and to the Whole; ſo that it may be increaſed by Addition of like Parts, and diminiſhed by Subtraction, may be multiplied and divided, and in a Word, may bear any Proportion to another

2 Quantity

Quantity of the fame kind, that one Line or Number can bear to another. That this is effential to all mathematical Quantity, is evident from the firft Elements of Algebra, which treats of Quantity in general, or of thofe Relations and Properties which are common to all Kinds of Quantity. Every algebraical Quantity is fuppofed capable not only of being increafed and diminifhed, but of being exactly doubled, tripled, halfed, or of bearing any affignable Proportion to another Quantity of the fame kind. This then is the Characteriftic of Quantity; whatever has this Property may be adopted into Mathematics; and its Quantity and Relations may be meafured with mathematical Accuracy and Certainty.

SECT. 2.

Of Proper and Improper Quantity.

There are fome Quantities which may be called *Proper*, and others *Improper*. This Diftinction is taken notice of by *Ariftotle*; but it deferves fome Explication.

I call that *Proper* Quantity which is meafured by its own Kind; or which of its own Nature is capable of being doubled or tripled, without taking in any Quantity of a different Kind as a Meafure of it. Thus a Line is meafured by known Lines, as Inches, Feet, or Miles; and the Length of a Foot being known, there can be no Queftion about the Length of two Feet, or of any Part or Multiple of a Foot. And this known Length, by being multiplied or divided, is fufficient to give us a diftinct Idea of any Length whatfoever.

Improper Quantity is that which cannot be meaſured by its own Kind; but to which we aſſign a Meaſure by the means of ſome proper Quantity that is related to it. Thus Velocity of Motion, when we conſider it by itſelf, cannot be meaſured. We may perceive one Body to move faſter, another ſlower; but we can have no diſtinct Idea of a Proportion or *Ratio* between their Velocities, without taking in ſome Quantity of another Kind to meaſure them by. Having therefore obſerved, that by a greater Velocity a greater Space is paſſed over in the ſame time, by a leſs Velocity a leſs Space, and by an equal Velocity an equal Space; we hence learn to meaſure Velocity by the Space paſſed over in a given Time, and to reckon it to be in exact Proportion to that Space: And having once aſſigned this Meaſure to it, we can then, and not till then, conceive one Velocity to be exactly double, or half, or in any other Proportion to another; we may then introduce it into mathematical Reaſoning without Danger of Confuſion, or Error, and may alſo uſe it as a Meaſure of other Improper Quantities.

All the Kinds of Proper Quantity we know, may, I think, be reduced to theſe four, Extenſion, Duration, Number, and Proportion. Tho' Proportion be meaſurable in its own Nature, and therefore hath Proper Quantity, yet as Things cannot have Proportion which have not Quantity of ſome other Kind, it follows, that whatever has Quantity muſt have it in one or other of theſe three Kinds, Extenſion, Duration, or Number. Theſe are the Meaſures of themſelves, and of all Things elſe that are meaſurable.

Number

Number is applicable to some things, to which it is not commonly applied by the Vulgar. Thus, by attentive Consideration, Lots and Chances of various Kinds appear to be made up of a determinate Number of Chances that are allowed to be equal; and by numbering these, the Values and Proportions of those which are compounded of them may be demonstrated.

Velocity, the Quantity of Motion, Density, Elasticity, the *Vis insita*, and *impressa*, the various Kinds of centripetal Forces, and different Orders of Fluxions, are all Improper Quantities; which therefore ought not to be admitted into Mathematics, without having a Measure of them assigned. The Measure of an improper Quantity ought always to be included in the Definition of it; for it is the giving it a Measure that makes it a proper Subject of mathematical Reasoning. If all Mathematicians had considered, this as carefully as Sir *Isaac Newton* appears to have done, some Labour had been saved both to themselves and to their Readers. That Great Man, whose clear and comprehensive Understanding appears, even in his Definitions, having frequent Occasion to treat of such improper Quantities, never fails to define them, so as to give a Measure of them, either in proper Quantities, or in such as had a known Measure. This may be seen in the Definitions prefixed to his *Princip. Phil. Nat. Math.*

It is not easy to say how many Kinds of improper Quantity, may in time, be introduced into Mathematics, or to what new Subjects Measures may be applied: But this I think we may conclude,

 that

that there is no Foundation in Nature for, nor can any valuable End be served by applying Meaſure to any, thing but what has theſe two Properties. Firſt it muſt admit of Degrees of greater and leſs. Secondly, it muſt be aſſociated with, or related to ſomething that has proper Quantity, ſo as that when one is increaſed the other is increaſed, when one is diminiſhed, the other is diminiſhed alſo; and every Degree of the one muſt have a determinate Magnitude or Quantity of the other correſponding to it.

It ſometimes happens, that we have Occaſion to apply different Meaſures to the ſame thing. Centripetal Force, as defined by *Newton*, may be meaſured various Ways, he himſelf gives different Meaſures of it, and diſtinguiſhes them by different Names, as may be ſeen in the above-mentioned Definitions.

In reality, I conceive that the applying of Meaſures to things that properly have not Quantity, is only a Fiction or Artifice of the Mind, for enabling us to conceive more eaſily, and more diſtinctly to expreſs and demonſtrate, the Properties and Relations of thoſe things that have real Quantity. The Propoſitions contained in the two firſt Books of *Newton's Principia* might perhaps be expreſſed and demonſtrated, without thoſe various Meaſures of Motion, and of centripetal and impreſſed Forces which he uſes: But this would occaſion ſuch intricate and perplexed Circumlocutions, and ſuch a tedious Length of Demonſtrations as would fright any ſober Perſon from attempting to read them.

4 Sect.

SECT. 3. *Coroll. first.*

From the Nature of Quantity we may see what it is that gives Mathematics such Advantage over other Sciences, in Clearness and Certainty; namely, that Quantity admits of a much greater Variety of Relations than any other Subject of human Reasoning; and at the same time every Relation or Proportion of Quantities may by the Help of Lines and Numbers be so distinctly defined, as to be easily distinguished from all others, without any Danger of Mistake. Hence it is that we are able to trace its Relations through a long Process of Reasoning, and with a Perspicuity and Accuracy which we in vain expect in Subjects not capable of Mensuration.

Extended Quantities, such as Lines, Surfaces and Solids, besides what they have in common with all other Quantities, have this peculiar, That their Parts have a particular Place and Disposition among themselves: A Line may not only bear any assignable Proportion to another, in Length or Magnitude, but Lines of the same Length may vary in the Disposition of their Parts; one may be streight, another may be Part of a Curve of any Kind or Dimension, of which there is an endless Variety. The like may be said of Surfaces and Solids. So that extended Quantities admit of no less Variety with regard to their Form than with regard to their Magnitude: And as their various Forms may be exactly defined and measured, no less than their Magnitudes, hence it is that Geometry, which treats of extended Quantity, leads us into a much greater Compass and Variety of Reasoning than any other Branch

of

of Mathematics. Long Deductions in Algebra for the moſt part are made, not ſo much by a Train of Reaſoning in the Mind, as by an artificial kind of Operation, which is built on a few very ſimple Principles: But in Geometry we may build one Propoſition upon another, a third upon that, and ſo on, without ever coming to a Limit which we cannot exceed. The Properties of the more ſimple Figures can hardly be exhauſted, much leſs thoſe of the more complex ones.

SECT. 4. *Coroll.* 2.

It may I think be deduced from what hath been above ſaid, That mathematical Evidence is an Evidence *ſui generis*, not competent to any Propoſition which does not expreſs a Relation of Things meaſurable by Lines or Numbers. All proper Quantity may be meaſured by theſe, and improper Quantities muſt be meaſured by thoſe that are proper.

There are many Things capable of More and Leſs, which perhaps are not capable of Menſuration. Taſtes, Smells, the Senſations of Heat and Cold, Beauty, Pleaſure, all the Affections and Appetites of the Mind, Wiſdom, Folly, and moſt Kinds of Probability, with many other Things too tedious to enumerate, admit of Degrees, but have not yet been reduced to Meaſure, nor, as I apprehend, ever can be. I ſay, moſt Kinds of Probability, becauſe one Kind of it, *viz.* the Probability of Chances is properly meaſurable by Number, as is above obſerved.

Altho' Attempts have been made to apply mathematical Reaſoning to ſome of theſe Things, and the Quantity of Virtue and Merit in Actions has been meaſured

meaſured by ſimple and compound *Ratio's*; yet I do not think that any real Knowlege has been ſtruck out this Way: It may perhaps, if diſcretely uſed, be a Help to Diſcourſe on theſe Subjects, by pleaſing the Imagination, and illuſtrating what is already known; but until our Affections and Appetites ſhall themſelves be reduced to Quantity, and exact Meaſures of their various Degrees be aſſigned, in vain ſhall we eſſay to meaſure Virtue and Merit by them. This is only to ring Changes upon Words, and to make a Shew of mathematical Reaſoning, without advancing one Step in real Knowlege.

SECT. 5. *Coroll.* 3.

I apprehend the Account that hath been given of the Nature of proper and improper Quantity may alſo throw ſome Light upon the Controverſy about the Force of moving Bodies, which long exerciſed the Pens of many Mathematicians, and for what I know is rather drop'd than ended; to the no ſmall Scandal of Mathematics, which hath always boaſted of a Degree of Evidence, inconſiſtent with Debates that can be brought to no Iſſue.

Tho' Philoſophers on both Sides agree with one another, and with the Vulgar in this, That the Force of a moving Body is the ſame, while its Velocity is the ſame, is increaſed when its Velocity is increaſed, and diminiſhed when that is diminiſhed. But this vague Notion of Force, in which both Sides agree, tho' perhaps ſufficient for common Diſcourſe, yet is not ſufficient to make it a Subject of mathematical Reaſoning: In order to that, it muſt be more accurately defined, and ſo defined as to give

us

us a Meaſure of it, that we may underſtand what is meant by a double or a triple Force. The *Ratio* of one Force to another cannot be perceived but by a Meaſure; and that Meaſure muſt be ſettled not by mathematical Reaſoning, but by a Definition. Let any one conſider Force without relation to any other Quantity, and ſee whether he can conceive one Force exactly double to another; I am ſure I cannot, nor ſhall, till I ſhall be endowed with ſome new Faculty; for I know nothing of Force but by its Effects, and therefore can meaſure it only by its Effects. Till Force then is defined, and by that Definition a Meaſure of it aſſigned, we fight in the dark about a vague Idea, which is not ſufficiently determined to be admitted into any mathematical Propoſition. And when ſuch a Definition is given, the Controverſy will preſently be ended.

SECT. 6.

Of the Newtonian Meaſure of Force.

You ſay, the Force of a Body in Motion is as its Velocity: Either you mean to lay this down as a Definition as *Newton* himſelf has done; or you mean to affirm it as a Propoſition capable of Proof. If you mean to lay it down as a Definition, it is no more than if you ſhould ſay, I call that a double Force which gives a double Velocity to the ſame Body, a triple Force which gives a triple Velocity, and ſo on in Proportion. This I intirely agree to; no mathematical Definition of Force can be given that is more clear and ſimple, none that is more agreeable to the common Uſe of the Word in Language.

For,

For ſince all Men agree, that the Force of the Body being the ſame, the Velocity muſt alſo be the ſame; the Force being increaſed or diminiſhed, the Velocity muſt be ſo alſo, what can be more natural or proper than to take the Velocity for the Meaſure of the Force?

Several other things might be advanced to ſhew that this Definition agrees beſt with the common popular Notion of the Word Force. If two Bodies meet directly with a Shock, which mutually deſtroys their Motion without producing any other ſenſible Effect, the Vulgar would pronounce, without Heſitation, that they met with equal Force; and ſo they do, according to the Meaſure of Force above laid down: For we find by Experience, that in this Caſe their Velocities are reciprocally as their Quantities of Matter. In Mechanics, where by a Machine two Powers or Weights are kept in *equilibrio*, the Vulgar would reckon that theſe Powers act with equal Force, and ſo by this Definition they do. The Power of Gravity being conſtant and uniform, any one would expect that it ſhould give equal Degrees of Force to a Body in equal Times, and ſo by this Definition it does. So that this Definition is not only clear and ſimple, but it agrees beſt with the Uſe of the Word Force in common Language, and this I think is all that can be deſired in a Definition.

But if you are not ſatisfied with laying it down as a Definition, that the Force of a Body is as its Velocity, but will needs prove it by Demonſtration or Experiment; I muſt beg of you, before you take one Step in the Proof, to let me know what you

 mean

mean by Force, and what by a double or a triple Force. This you muſt do by a Definition which contains a Meaſure of Force. Some primary Meaſure of Force muſt be taken for granted, or laid down by way of Definition; otherwiſe we can never reaſon about its Quantity. And why then may you not take the Velocity for the primary Meaſure as well as any other? You will find none that is more ſimple, more diſtinct, or more agreeable to the common Uſe of the Word Force: And he that rejects one Definition that has theſe Properties, has equal Right to reject any other. I ſay then, that it is impoſſible, by mathematical Reaſoning or Experiment, to prove that the Force of a Body is as its Velocity, without taking for granted the thing you would prove, or ſomething elſe that is no more evident than the thing to be proved.

SECT. 7.

Of the Leibnitzian *Meaſure of Force.*

Let us next hear the *Leibnitzian*, who ſays, that the Force of a Body is as the Square of its Velocity. If he lays this down as a Definition, I ſhall rather agree to it, than quarrel about Words, and for the future ſhall underſtand him, by a quadruple Force to mean that which gives a double Velocity, by 9 times the Force that which gives three times the Velocity, and ſo on in duplicate Proportion. While he keeps by his Definition, it will not neceſſarily lead him into any Error in Mathematics or Mechanics. For, however paradoxical his Concluſions may appear, however different in Words from theirs who

who meaſure Force by the ſimple *Ratio* of the Velocity; they will in their Meaning be the ſame: Juſt as he who would call a Foot twenty-four Inches, without changing other Meaſures of Length, when he ſays a Yard contains a Foot and a half, means the very ſame as you do, when you ſay a Yard contains three Feet.

But tho' I allow this Meaſure of Force to be diſtinct, and cannot charge it with Falſhood, for no Definition can be falſe, yet I ſay in the firſt place, it is leſs ſimple than the other; for why ſhould a duplicate *Ratio* be uſed where the ſimple *Ratio* will do as well? In the next place, this Meaſure of Force is leſs agreeable to the common Uſe of the Word Force, as hath been ſhewn above; and this indeed is all that the many laboured Arguments and Experiments, brought to overturn it, do prove. This alſo is evident, from the Paradoxes into which it has led its Defenders.

We are next to conſider the Pretences of the *Leibnitzian*, who will undertake to prove by Demonſtration, or Experiment, that Force is as the Square of the Velocity. I ask him firſt, what he lays down for the firſt Meaſure of Force? The only Meaſure I remember to have been given by the Philoſophers of that Side, and which ſeems firſt of all to have led *Leibnitz* into his Notion of Force, is this: The Height to which a Body is impell'd by any impreſſed Force, is, ſays he, the whole Effect of that Force, and therefore muſt be proportional to the Cauſe: But this Height is found to be as the Square of the Velocity which the Body had at the Beginning of its Motion.

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In this Argument I apprehend that great Man has been extremely unfortunate. For, *1st*, Whereas all Proof ſhould be taken from Principles that are common to both Sides, in order to prove a thing we deny, he aſſumes a Principle which we think farther from the Truth; namely, that the Height to which the Body riſes is the whole Effect of the Impulſe, and ought to be the whole Meaſure of it. *2dly*, His Reaſoning ſerves as well againſt him as for him: For may I not plead with as good Reaſon at leaſt thus? The Velocity given by an impreſſed Force is the whole Effect of that impreſſed Force; and therefore the Force muſt be as the Velocity. *3dly*, Suppoſing the Height to which the Body is raiſed to be the Meaſure of the Force, this Principle overturns the Concluſion he would eſtabliſh by it, as well as that which he oppoſes. For, ſuppoſing the firſt Velocity of the Body to be ſtill the ſame; the Height to which it riſes will be increaſed, if the Power of Gravity is diminiſhed; and diminiſhed, if the Power of Gravity is increaſed. Bodies deſcend ſlower at the Equator, and faſter towards the Poles, as is found by Experiments made on Pendulums. If then a Body is driven upwards at the Equator with a given Velocity, and the ſame Body is afterwards driven upwards at *Leipſick* with the ſame Velocity, the Height to which it riſes in the former Caſe will be greater than in the latter; and therefore, according to his Reaſoning, its Force was greater in the former Caſe; but the Velocity in both was the ſame; conſequently the Force is not as the Square of the Velocity any more than as the Velocity.

SECT.

SECT. 8.

Reflections on this Controversy.

Upon the whole, I cannot but think the Controvertists on both Sides have had a very hard Task; the one to prove, by mathematical Reasoning and Experiment, what ought to be taken for granted, the other by the same means to prove what might be granted, making some Allowance for Impropriety of Expression, but can never be proved.

If some Mathematician should take it in his Head to affirm, that the Velocity of a Body is not as the Space it passes over in a given Time, but as the Square of that Space; you might bring mathematical Arguments and Experiments to confute him; but you would never by these force him to yield, if he was ingenuous in his Way; because you have no common Principles left you to argue from, and you differ from one another, not in a mathematical Proposition, but in a mathematical Definition.

Suppose a Philosopher has consider'd only that Measure of centripetal Force which is proportional to the Velocity generated by it in a given Time, and from this Measure deduces several Propositions. Another Philosopher in a distant Country, who has the same general Notion of centripetal Force, takes the Velocity generated by it, and the Quantity of Matter together, as the Measure of it. From this he deduces several Conclusions, that seem directly contrary to those of the other. Thereupon a serious Controversy is begun, whether centripetal Force be as the Velocity, or as the Velocity and Quantity of Matter taken together. Much mathematical and

experimental Duſt is raiſed; and yet neither Party can ever be brought to yield; for they are both in the right, only they have been unlucky in giving the ſame Name to different mathematical Conceptions. Had they diſtinguiſhed theſe Meaſures of centripetal Force as *Newton* has done, calling the one *Vis centripetæ Quantitatis acceleratrix*, the other *Quantitas motrix*; all Appearance of Contradiction had ceaſed, and their Propoſitions, which ſeem ſo contrary, had exactly tallied.

VI. *A Letter from* Rich. Haſſel *Eſq*; *F. R. S.* *to* Peter Daval *Eſq*; *Secr. R. S. concerning a large Piece of a* Lath *being thruſt into a Man's* Eye, *who recover'd of it.*

S I R,

Read Nov. 10. 1748.

I THOUGHT the following Caſe ſo extraordinary as to be worth the Notice of the *Royal Society*. If you think ſo too, I beg you to communicate it.

On *Sunday* the 17th of *Jan.* 1747. *Henry Halſey*, of *South Mims*, Labourer, thruſt a long Lath with great Violence into the great *Canthus* of the left Eye of *Edward Roberts* of the ſame Place Labourer, which broke off quite ſhort; ſo that a Piece two Inches and near a half long, half an Inch wide, and above a quarter of an Inch thick, (*ſee* TAB. I.) remained in his Head, and was ſo deeply buried there, that it could ſcarce

be

be ſeen, or laid hold of. He rode with the Piece of Lath in him from *Kick's End*, where the thing was done, to *Barnet*, which is above a Mile, to the Houſe of Mr. *Juſtinian Morſe*, a Surgeon there, who extracted it with Difficulty; it ſticking ſo hard, that others had been baffled in attempting to extract it. *Roberts* continued dangerouſly ill a long time; but at laſt, by the Bleſſing of God, and the Care of Mr. *Morſe*, recover'd intirely, and has the Sight of the Eye, and the Uſe of the Muſcles. But ſome time after he ſeemed well, he told me, that, upon leaning down forward, he felt great Pains in his Head. The Piece is ſuppoſed to have paſſed behind the right Eye. I am,

S I R,

Lincolns-Inn, Nov. 10. 1748.

Your humble Servant,

Richard Haſſel.

VII. *The* Sun's Eclipſe *of* July 14, 1748. *obſerved at* Marlborough Houſe, *with the twelve Foot refracting Teleſcope, fix'd as a Finder to the Tube of the great twelve Foot Reflector; by* John Bevis *M. D.*

Read Nov. 10. 1748.

	Apparent Time.			
	d	h	'	"
July	13.	9.	3.	50 The Beginning, which perhaps might be 2" or 3" ſooner.

Apparent Time.				
d	h	′	″	
July 13.	9.	39.	42.	The firſt little Spot in the Weſtern Cluſter, quite covered.
		52.	00.	The biggeſt of that Cluſter quite cover'd, yet ſomewhat doubtful for flying Clouds.
	10.	12.	08.	The middle one of three conſiderable Spots towards the Eaſtern Limb half cover'd.
				The End could not be preciſely obſerved for flying Clouds; at 12. 09. 15. it was not quite over; but at 12. 09. 35. the Sun was clear, and nothing of the Eclipſe left.

N. B. *The Wind was ſo boiſterous, that no Phaſes could be meaſured with a Micrometer.*

The Moon's Eclipſe *of* July 28. 1748. *obſerv'd at the ſame Place.*

d	h	′	″	
July 28.	10.	13.	28.	The *Penumbra* diſcernible.
		06.	30.	The Beginning, as moſt of the Company judged.
		18.	38.	*Mare Humorum* juſt touch'd.
		26.	24.	Began to touch *Tycho*.
		27.	51.	*Tycho* biſected.
		24.	09.	*Tycho* cover'd.
		29.	53.	Touch'd *Grimaldi*.
		30.	25.	*Mare Humorum* cover'd.
		34.	14.	*Grimaldi* cover'd.

July

	h.	′	″	
July 28.	12.	24.	30.	The End.
		27.	40.	The *Penumbra* quite gone.

About the Middle of the Eclipſe, the Moon's Diameter, perpendicular to the Equator, meaſur'd in a 5 Foot Teleſcope was 33′ 50″; perhaps 15″ or 20″ greater than it would have been found to be with a 12 Foot Tube.

J. Bevis.

I lately received the following Letter from a Perſon unknown to me.

S I R, *Luffwick* near *Thrapſton Northamptonſhire.*

I HAD the Pleaſure to obſerve the Sun's Eclipſe *July* laſt, which was as follows. The Beginning 9h. 1′. 0″. *a. m.* The End 0. 5. 25. *p. m.* at 10h. 32″. 10′. *a. m.* 10°. 18′. were dark, which I take to be the greateſt with us. Theſe are apparent Times, from a well adjuſted Clock (by a Meridian drawn *June* 10, on a Plate of Metal), and corrected to the Time of Obſervation.

Our Latitude is 52°. 27′. 30″. I hope by this, and future Obſervations, I ſhall be able to determine our Situation with reſpect to yours. I am,

S I R, &c.

Oct. 21. 1748.

Mark Day.

VIII. *An Observation of an extraordinary Lunar* Circle, *and of two* Parafelene's, *made at* Paris, Oct. 20. 1747. N. S. *and of the* Eclipfe *of the* Sun, July 14. 1748. O. S. *by* Auguftine Nathaniel Grefchow, *Memb. of the* Royal Acad. *of* Sciences *at* Berlin, *&c.*

Tranflated from the French *by* T. S. *M. D.*

Read Nov. 10. 1748.

OCTOBER 20. at Night, the Sky was darkened by a flight Fog, thro' which the Moon appeared of a firey-red Colour, till $8^h. 40^m$. when the Fog was thoroughly difperfed, and the Heavens were overcaft with a whitifh ftreaky Cloud. At the fame time there appeared round the Moon a Halo (TAB. II. *Fig.* 7. *ABCD*) accompanied with four other Segments of Circles, two of which *EAF* and *GH* of ten Degrees, were concentric, fo as to have their common Center at the Zenith. The Segment or Arch *IPL* on the North Side, of feven Degrees, was concentric with the great lunar Circle, and confequently had the Moon for its Centre; and in fine the Arch *MCN*, which faced the Horizon, was of twelve Degrees.

Befides thefe four Segments, what was moft remarkable was a Mock-Moon or *Parafelene B*, fhaped like a Mock-Sun or *Parhelius*. The Diameter of this Mock-Moon, tho' ill-determin'd, was of 35 Minutes at leaft, with a Tail *BP* oppofite to the Moon, as the Tail of a Comet is oppofite to the Sun. This Tail varied in its Degree of Light from time to time, extending as far as the Arch *IPL*, which,

which, as well as the Arch *GH*; was 4 Degrees diſtant from the lunar Circle *ABCD*. The *Paraſelene B* had the ſame Colours with a common *Parhelius*, excepting that they were not ſo lively, but they very much inclined to the tawny, eſpecially on the Side, which faced the Moon. This *Paraſelene* was in the ſame Altitude as the Moon. Its Tail was much more faint and tranſparent; inaſmuch as *Capella* appeared thro' this luminous Tail. The lunar Circle *ABCD* was much weaker to the South, and there appeared no *Paraſelene* on that Side. This Meteor did not ſeem to undergo any Alteration till 9^h. 18'. when the Atmoſphere was cover'd with thick Clouds.

The Clouds being diminiſhed at 9^h. 32'. the Meteor appeared again, but very different from what it was before; for, inſtead of ſeeing a lunar Circle with 4 other Arches of Circles, I ſaw the lunar Circle *DABD*, and on the South Side a faint Arch *QR* of four Degrees, having the Moon for its Centre in common with the great lunar Circle. There were likewiſe two *Paraſelene's*, one of which *B* was to the North, and the other *D* to the South, as they are expreſſed in *Fig*. 8. Theſe two *Paraſelene's* did not caſt ſo ſtrong a Light as that which had appeared before, nor were they ſo diſtinctly formed. On the contrary, the lunar Circle was very beautiful, and remarkably bright, until 9^h. 50'. when the whole *Phænomenon* diſappeared, and the Sky grew clear by degrees. The Moon's Diameter was 30'. 30''. On the ſame Night a very beautiful lunar Circle was obſerved at *Berlin*, but without *Paraſelene's*.

The following is the Obſervation that was made of the laſt Eclipſe of the Sun, at the Obſervatory Royal *at* Berlin.

1748. *July* 25. *N. S.* The Beginning of the Eclipſe was not obſerved, the Sun having been covered with Clouds.

	h	′	″	
The *Annulus* was completed at	11	52	51	*ante merid.*
—— —— —— broken	11	54	13	
The End of the Eclipſe	1	25	9	*poſt. merid.*

The Diameter of the Sun was 31′. 43″.

This Eclipſe was likewiſe obſerved annular at *Francfort* upon the *Oder*, but not ſo exactly as at *Berlin.*

IX. *A Letter from* James Parſons *M. D. F. R. S. to the* Preſident, *containing an Account of a preternatural Conjunction of two* Female Children.

SIR,

Read Nov. 17. 1748.

ABOUT the middle of *September* laſt a Woman in *Holborn* was deliver'd with much Difficulty of two Girls join'd together by the Bellies in ſo ſingular a manner, as to deſerve a particular Deſcription to be laid before you and the *Society*, as a very curious Subject.

The Care of preparing theſe Children for keeping in Spirits was committed to Mr. *James Sherwood* Surgeon, who was ſo kind to ſend for me to obſerve them with him; and it was reſolved to inject them, in order to make our anatomical Examination the more accu-

rately,

rately, which was very ingeniously executed by Mr. *Sherwood*, and the State of the Children was as follows:

The Skin of Part of the Breast and Belly was continued to each Child, from the lower Part of the *Sternum*, down to the Insertion of a single * *Funis umbilicalis*, which, instead of one to each, serves in common to both.

Each Child had its peculiar Muscles of the *Abdomen*; but the strait Muscles were so divided, as that the *Rectus* on the right Side of the one Child had the *Linea alba* between it and the *Rectus* on the left Side of the other, and *vice versa*; so that the Line of each lying directly upon each other, was colliquated and open'd, and the Conjunction of the *Musculi recti*, thus formed but one common abdominal Cavity up to the Diaphragms of each Child; above which each had its own proper *Thorax*, even evident from their external Appearance; whereas, had their Junction been but never so little in a lateral Way, each would undoubtedly have had its own separate *Abdomen*, since they would not have been so closely pressed forwards, as to occasion that intimate Coalescion of Parts in the Subject before you; which is manifest in the Dissections of several of these kinds of Monstrosities, some of which have been join'd by the Hips, some by the Backs, some partly by the Sides, and one or two Cases mentioned by *Parée* and *Tulpius* joined by the Bellies.

None of these uncommon Subjects ought to be touch'd with a Knife, until it is well injected, because the vascular System, where there are any preternatural Adhesions

* See a similar Case in these *Trans.* N°. 65, p. 296.

Adhesions or Distortions, can never be understood nor traced without it; and therefore *Tulpius*, whose Account of his Subjects is very inaccurate, and who certainly did not inject it, confesses he could make no Distribution of the Vessels, nor find out any thing of them distinctly.

But in the present Case, a complete Injection of the Children being made by the Vessels of the umbilical Cord, we were enabled to give the following exact Account of the vascular System and other Parts; to which however we shall premise a Description of the intestinal Canal of both.

When we came to examine the Intestines, the only proper means for laying them fairly to View, before they were taken out of the Body, was to inflate them; which was accordingly done, and thereby every Part of them was rendered as conspicuous as the Drawing now before you, and of the same Size exactly. Each Child had its own peculiar *Oesophagus*, *Stomach*, and *Pylorus*, in a natural State; from each of which the *Duodenum* descended about three Inches, and then united into one common Duct, which we shall call the Beginning of the *Jejunum*, and which was near four Inches long: This was inserted into the upper Part of a large *Sacculus*, formed out of the very Coats of the Intestines, and differing in no wise from them in Colour, Density, or any other Quality but the Form and Extension.

Its horizontal Diameter was about 5 Inches, and its vertical about 4, and it was formed out of the *Jejunum*, which, in some Subjects, is as long as the *Ileum*, in most near that Length, and no doubt was

was an Attempt of Nature to ſupply the Want of two regular *Jejuna:* For we are to obſerve, that if theſe Children had lived, each having its own proper Stomach, would probably have eaten a due Quantity of Food for its Suſtenance; and the Office of the Stomachs might have been well enough performed; but each requiring a ſeparate Syſtem of Inteſtines to diſpoſe naturally of the digeſted Chyle, and this preternatural Conjunction happening between them, the *Jejuna* of both were confuſed together; and having Room in the *Abdomen*, now large and common to both, theſe Parts of their Organizations, that ought to have grown into two Guts of a conſiderable Length, being hindered from a regular Accretion, the joint growing Powers of both formed the Sack of Communication now before you; which is proportionably capacious enough to anſwer the Purpoſes of two natural *Jejuna*; below which the reſt of the Inteſtines of each Child were ſufficient to do their ſeveral Offices.

In the lower Part of this *Sacculus* there was an Outlet on each Side, which were the Origins of their ſeparate *Ilea:* Theſe were in a good State, and regularly inſerted each into its *Cæcum*; and this in each had its natural *Appendicula*; theſe were regularly ſucceeded by their *Colons*, and terminated by their proper *Recta inteſtina* to their natural Outlets; with this Difference only, that the *Colons* were out of their natural Situation, and were convoluted in each Child, by as narrow Portions of the *Meſocolon*, as any Part of the *Ileum* is by its Meſentery; and that as low as the going off of the *Rectum*.

Of

Of the Vascular System.

We are to take notice, that as these Children had but one abdominal Cavity between them, so it contained, in Appearance, only one Liver of a considerable Size, and an irregular Form; but this consisted of two in Reality preternaturally joined, as there were two Gall-Bladders. The umbilical Vein is inserted into this, pretty nearly in the usual manner, and afterwards this Canal is divided into two Branches, which carry the Blood into the *Vena cava* of each Child; whence it falls naturally into the right Auricles of their Hearts.

The Heart of the larger Child is but small, has a bifid *Apex*, and from the Division has a Vestige of the *Septum*, on both the upper and under Sides; which forms a *Sulcus* in a longitudinal Direction, from between the *Apices* to the *Basis*; from whence arises a *Pericardium* which extends itself over each Side from the *Sulcus*, and so forms a separate *Capsula* over each Ventricle of this Heart, and may therefore be call'd a double *Pericardium*.

The ascending Vessels are distributed according to the Standard of Nature; but the descending Trunk of the *Vena cava* rides over that of the *Aorta*, above the going off of the Emulgents, and sinks back again behind the external *Iliac Artery*, before it is itself divided into *Iliac Veins*, descending naturally to the lower Extremities, as do the Arteries from thence also.

The Kidneys, urinary and uterine Parts were in a natural State; and the Lungs appeared well, and seem'd as if this Child had breathed.

The

The Heart of the ſmaller Child was ſingle, but above a third larger than it naturally ought to be; out of which the aſcending Arteries are very regular and natural; but there was ſcarce any Veſtige of Lungs in this Child on the right Side of the *Thorax*, and but a ſmall Portion of the pulmonary Subſtance in the left. The deſcending Trunk of the *Aorta* is very ſmall in compariſon of the other; yet goes down regularly towards the Extremities, except the internal *Iliac Arteries*, which were obliterated and degenerated into Ligaments, whilſt the Externals continued down, as I have juſt ſaid; for only the *Iliacs* of the larger *Fœtus* took place in the umbilical Cord, which was the Reaſon that we found but two Arteries in it; ſo that, altho' both Children received Nouriſhment by the Diviſion of the Canal from the Liver to the *Venæ cavæ*, yet the ſuperfluous Blood of both could be ſent back to the *Placenta* no other way than by the internal *Iliacs* of the greater Child.

The deſcending Branches of the *Vena cava* enter'd as uſual, on the right Side, into the Auricles; but thoſe of the left join in one Trunk, paſs round the left Auricle, and enter into the right cloſe by the *Cava aſcendens*, which is of a natural Size, and very regular up to the Diaphragm, from which it extends a full Inch before it reaches the Auricle; the Kidneys differ a little in Size from each other; yet theſe, with the other urinary and alſo the uterine Parts in general, are in good Order; but the moſt remarkable *Luſus* of Nature in theſe Subjects is an Artery which ariſes from the *Aorta* about the Place of the *Celiac* of the one Child, running along

before the Liver, and is inserted into the same Place of the *Aorta* of the other. It was much larger than any other Artery in either Child, and bestow'd Branches on the *Stomach*, *Mesentery*, and *Meso-colon*; being about five Inches long; for there were neither *cæliac* nor *mesenteric* Arteries, according to Nature in either Child.

Observations.

When some former Authors (before the Learned were so happy as to know the Sweets of experimental Philosophy) endeavoured to account for monstrous Productions in the animal World, they could have recourse to no other means to explain them, than to the then reigning Systems by which they usually explained the *Phænomena* of the natural Generation of Animals; and that was in general, that an Animal was produced by the Admixtion of the supposed seminal Matter of both the Parents; that the Quantity produced by this Commixtion was supposed always to contain only a Quantity of Particles sufficient to produce a natural *Fœtus*, by the Mediation of a certain *plastic* Power, said always to attend it, as well as any other natural Production in the World.

Upon this Plan many little Alterations were made by succeeding Authors, without differing widely from this general Notion; all as liable to Objections, and as easily refuted as the Source from which they sprung. And notwithstanding the Truths that have since been traced out by later philosophical Advances, leading to a more reasonable Knowledge of the Subject, yet there still are some who

appear

appear unwilling to quit these old Errors. But as this is not the Place intended for a Discussion of the several Opinions concerning it, we shall here only consider how the Conjunction of these two Children happen'd; as well as the means whereby Children acquire superfluous or want the necessary Members, or are any otherwise deformed.

According to a late Reading I had the Honour to exhibit before you upon the Analogy between the Propagation of Animals and Vegetables *, I hope it appear'd pretty clearly, that both these Parts of the Creation are daily propagated from Organizations already formed and treasured up in natural Receptacles provided for them, till they come to be removed into proper Places of Nourishment; from which Opinion we can find no Reason to swerve at present, and which we must have recourse to, in order to account for the present Subject.

If the old Systems had Weight formerly, later Enquiries have exposed their Absurdities. Truth admits of nothing absurd, and as to what regards the Works of the Creation, especially that of Animals and Vegetables, that System of accounting for their Generation which is most simple, and is least liable to Objections, is most likely to succeed in the Enquiry; and since the Ordination of Providence was that all should be good, it will appear the means of the Propagation of Animals and Vegetables, which was partly the Subject of the Discourse lately read before you, will seem least of all liable to any Accidents that might degrade the ge-

* Which I design shortly to publish. *J. P.*

neral Usage and Standard of Nature, by the Production of Monstrosities in any Part of her Works.

When the *Vis plastica* was thought to be the Agent for the Guidance of the Work of Generation, and that a monstrous Child appear'd, it was blamed by the Authors of most Nations in some of these Particulars; the seminal Matter was either in too small or too great a Quantity, and the *Vis plastica* puzzled in the Management of the Particles that go to form the different Parts of the Animal; or it was sometimes careless and negligent in the Application of the Ideas of some or other of the Parts, and consequently the Animal must want some Limb or other; or, from a Superabundance of Matter, have superfluous Limbs added to them by this unskilful erring Agent: But in whatsoever manner the Commixtion of the male *Semen*, with that ignorantly supposed to be in Females, and the Formation of a *Fœtus* therefrom, is said to be conducted, the Accidents and Chances against the Welfare of all animal Beings would be so numerous, and the State of Nature so miserable, that the greatest Part of the Inhabitants of the Earth and Waters could not avoid being monstrous, and full of Confusion: The Almighty would have produced an Effect contrary to His Divine Goodness, and Care for His Creatures; and, in fine, it would be highly absurd, to suppose the Regulation of things of this high Consequence to be committed to any finite subordinate ignorant Agent, which must undeniably be insufficient for this great Work.

But the System of Generation which supposes the Organizations of Animals and Vegetables already formed with an Incapacity of growing into any

other

other Forms than thoſe of their Parents, is the moſt ſecure from any Confuſion, or any preternatural Digreſſions from their due Forms, is moſt compatible with that gracious Deſign, that all ſhould be good: For every animal and vegetable Body is daily ſeen to be conſtant to its own Kind, and can be ſubject to no Accident but one to render it monſtrous in its Accretion, and that in general is Compreſſion: For all animal and vegetable *Ova* are moſt certainly perfect in their firſt Formation, as the Seeds of the latter plainly ſhew, and in a State of Reſt, until they are depoſited in their natural *Matrix*, be it ſooner or later; the *Ova* of Females in the *Ovaria* during Life, and the Seeds of Vegetables in our Repoſitories for any Number of Years; and, after that, would certainly, without Accidents or Interruption, continue their perfect Form to their utmoſt Growth.

From hence it will be eaſy to account for the preternatural Adheſion of theſe two Children, and the Confuſion of their *Viſcera*, upon the moſt eaſy Plan, and moſt ſimple and perſuaſive Reaſoning imaginable; and from hence alſo we ſhall be able to account for every other Monſtroſity that can attend Animals and Vegetables,

We have obſerved before, that each Seed and *Ovum* contains the Animal and Vegetable proper to its Species. Now, when two or more of theſe animal *Ova* are fecundated, and come into the *Uterus*, the Sides of the *Ova* (which are the Membrances that contain the Fluids in which the little Organizations ſwim) muſt inevitably come into Contact; and if the Membranes of each continue in a good State, the *Fœtus's* will be free in their
ſeveral

ſeveral Apartments, and grow proportionably; but if the Parts of the Membranes, which are cloſe together, by being thin and weak, or by any irregular Reſiſtance, or Friction, come to be diſſolved or broken, then the Fluids of both unite, and the two little Organizations, having no longer a Partition between them, come together, adhere, and intwine into each other, their tender Parts eaſily coaleſcing; and from the natural Diſpoſition of each to grow and increaſe, their Accretion goes on, there is a mutual Inſinuation of Veſſels where the Parts are compreſſed, and a mutual confuſed Circulation carried on, and at length the Whole becomes irregular and monſtrous.

We have many Facts to corroborate this Opinion, and to ſhew that the Fibres of Animals and Vegetables have a wonderful Capacity of extending and inſinuating themſelves into one another; and of continuing a Circulation reciprocally; and the Blood-Veſſels of being elongated, and even of producing new Ramifications where the Reſtoration of a Part requires it, from the Principles I before laid down in the firſt Part of my Analogy; elſe how ſhould wenny Tumours of a monſtrous Size be propagated on the Surfaces of human Bodies? how ſhould the Lungs adhere to the *Pleura* ſo intimately as to become one entirely united Maſs, as incapable of being ſeparated without a Knife as any Part of a Muſcle? How ſhould the *Surculi* or *Buds* of Trees implanted into others by Grafting or Inoculation, ſo inſinuate their Fibres into thoſe of the Stock in which they grow, as to

become

become one continued Piece of Wood with them? How are the *Sutures* of the *Scull*, and those of the *Epiphyses* of Bones, totally obliterated in an advanced Age, but by the Insinuation and reciprocal Combination of the Fibres of each other? And, in a word, how are many recent deep Wounds so soon agglutinated, if there be not a speedy Insinuation of Vessels, and a Circulation soon carried on?

Indeed if we are only to look on, and consider the Subject before us in its present State, it will be somewhat difficult to conceive how this strange Conjunction could happen; but we are to go back, and consider two minute tender Organizations, whose remotest Parts from each other might not exceed perhaps the fiftieth Part of an Inch at the time of their Adhesion, and the Difficulty is taken away. Thus our Children happening to be compressed by their Bellies, the tender Integuments between the *Musculi recti* in each were soon thinned and dissolved, the Coalescion happen'd as I have said before, and the Intrusion and Commixtion of Parts, that appear before you, was begun and carried on by their growing wheresoever the Resistance was least.

And thus if the Contact and Pressure of the two *Fœtus's* be pretty equal and moderate, they will grow equally; if the Compression be very great, and both be compressed all round, having no Room, because of the *Uterus* not giving Way, the Confusion of both, or indeed of one only, will be so great, as to cause a Mass without any Form or Regularity at all. If their Contact be so disposed, as that one *Fœtus* is much compressed and confined, the other

has

has sufficient Room, this will grow proportionably, whilst the Growth of the other being intirely stopped from the Beginning in its minute State, except perhaps an Arm, or Leg, or Head, *&c.* that which has Liberty will have such superfluous Limb or Part growing with it, as remained uncompressed, whilst the rest is obliterared and lost.

The same is also apparent in Vegetables: A Carrot, Parsnip, Radish, and such-like Vegetables as naturally grow strait and well-form'd, may be distorted and alter'd at Pleasure, as they grow, by Compression: For, as the nutritious Juices are equally distributed, and attracted in the same Quantity for the Use of the Whole; if a Compression be made on any Part, those Juices, which are hinder'd to flow into the compressed Part, will be determined elsewhere, and form Gibbosities and Deformities in other Parts of the Organization, where the Resistance is less, and the Whole become changed from its natural Form. Thus *Gourds*, as they grow, in applying Pressure by Ligatures, or otherwise, may be brought to various Forms; and *Apples*, placed in cylindrical *Phials*, whilst small, will, by the lateral Pressure, lose their roundish Form, and acquire that of a *Cylinder*. And thus Nuts, Apples, *&c.* may be conjoin'd and become double; and not because there was a Superfluity of a supposed Matter to form them, by any subordinate Help: And thus also a Nut or Apple, *&c.* among a Bunch of sound ones, may, by Compression of its Organization, be vitiated and ill-form'd; and not because there wanted a Sufficiency of this supposed Matter.

In

TAB. III.

Fig. 1.
p. 539.

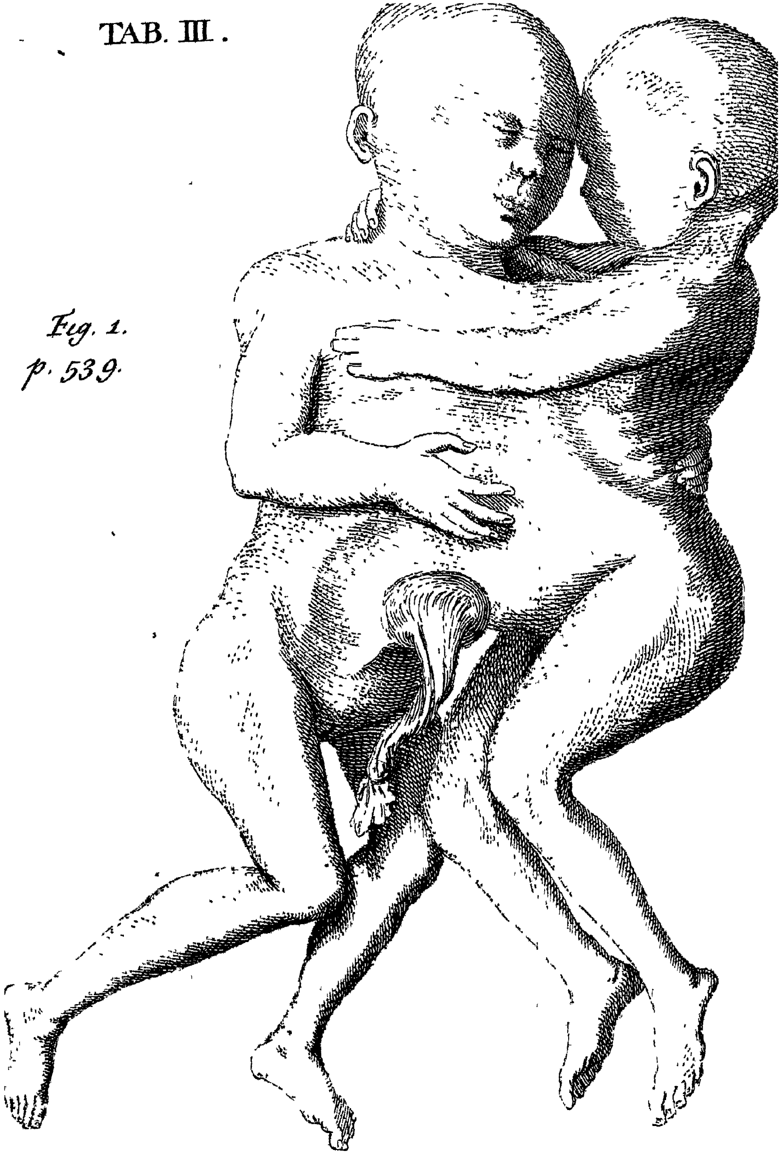
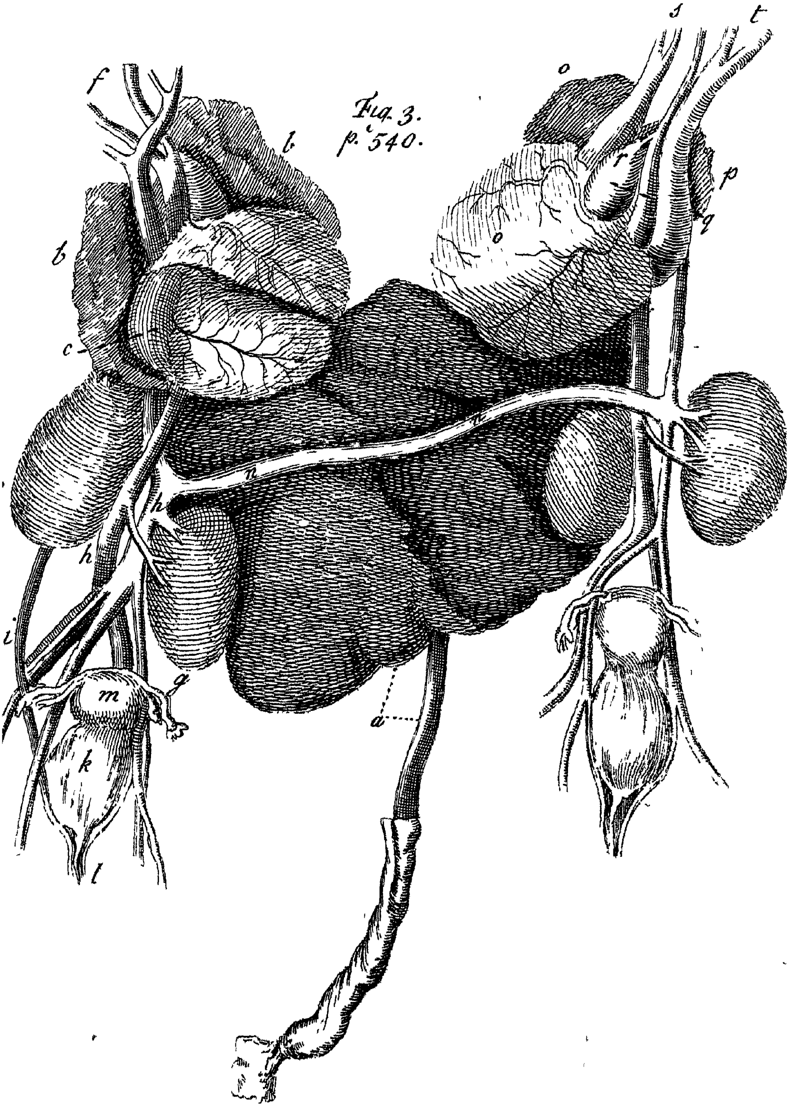


Fig. 3.
p. 540.



J. Mynde sc.

In this manner, it is highly probable, all animal Monſtroſities happen; and this was the Caſe of the *Cow*, which many of us ſaw a Fortnight ago. Her twin Siſter happen'd to be confin'd and compreſſed to her Spine; nothing remaining free but the *Abdomen*, the Dugs, one of the anterior Extremities, and the Dew-lap; every other Part was obliterated, whilſt theſe continued to grow, by the Communication of Veſſels between them, in the manner above explained.

This, Sir, is the Sum of what I am capable of conceiving concerning monſtrous Productions: I ſhall think myſelf happy in the Concurrence of this learned *Society* with my Opinion; and am, with the trueſt Reſpect,

S I R,

Your moſt obedient Servant,

James Parſons.

An Explanation of the Drawings repreſenting the above-deſcribed Two Children joined together.

Tab. III. *Fig.* 1.

Repreſents two Female Children preternaturally join'd by the abdominal Integuments, from the *Umbilicus* up to the *Cartilago enſiformis*, in ſuch a manner, as to form between them but one *Abdomen*.

TAB. IV. *Fig.* 2.

Shews a back View of the inteſtinal Canal of each Child, from the Stomach to the *Anus*.

a, *a*, The Stomach of each.
b, *b*, The *Duodenum*.
c, *c*, Part of the *Jejunum*, which is common to both Children.
d, The remaining Part of the *Jejunum* form'd into a *Sacculus*, out of which,
e, *e*, The *Ileum* of each Child ariſes.
f, *f*, The *Cæcum* of each.
g, *g*, The *Colon* of each.
h, *h*, The *Rectum* of each.

TAB. III. *Fig.* 3.

Is a fore View of the other *Viſcera*, and vaſcular Syſtem of both Children.

a, the umbilical Vein entering into the Liver, which is form'd of that of each Child preternaturally conjoin'd.
b, *b*, The Lungs of the larger Child.
c, The Heart, which has a bifid *Apex*, and of which each Ventricle has its particular *Pericardium*, from the *Sulcus* that divided it.
f, The great Veſſels ariſing out of the Heart.
g, *g*, The Kidneys in ſome meaſure misſhapen.
h, *h*, The Trunks of the large Veſſels deſcending to the lower Extremities. The Vein running before the Artery, and ſinking behind it again where it divides.
i, The right *Ureter*.

k,

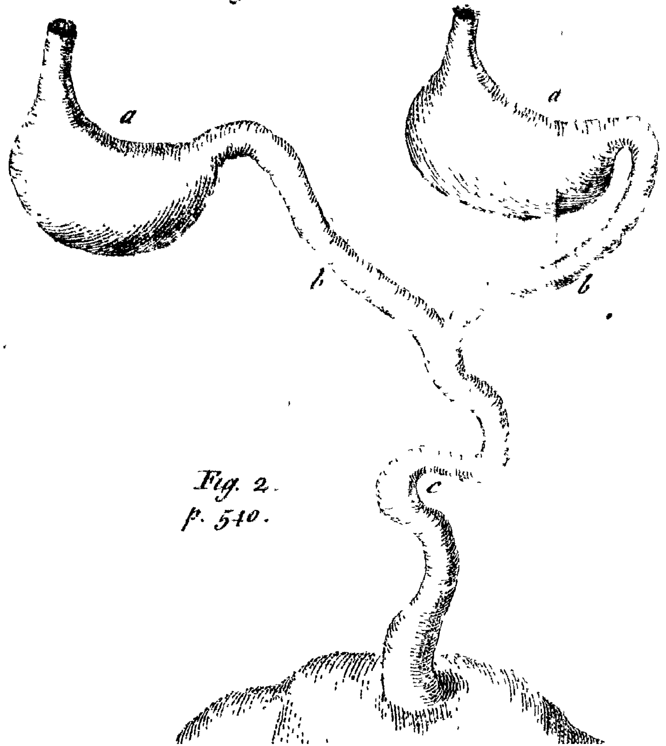
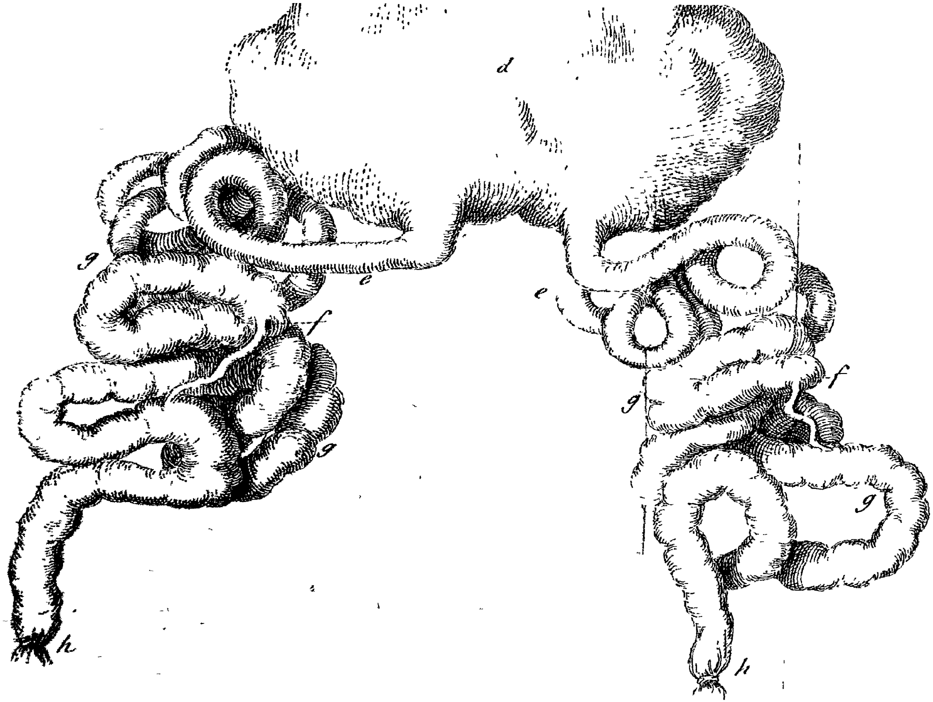


Fig. 2.
p. 540.



J Mynde sc

k, The Bladder; and

l, The umbilical Arteries, with the *Urachus* turn'd down, to shew

m, The *Uterus*, &c.

n, *n*, Is an Artery communicating with, and entering into, the *Aorta* of each, near the going off of the Emulgents.

o, The Heart of the smaller Child, much too large in proportion, together with the right Auricle.

p, Part of the Lungs, which were render'd much too small, in proportion, by the Compression of the large Heart upon them.

q, The *Aorta* and pulmonary Artery, as they are connected by the *Canalis Arteriosus* (*r*).

s, *t*, The descending Trunks of the Veins; the latter of which was preternatural, running round the left, and entering into the right Auricle in its posterior Part.

The other Parts were much as those of the former Child in general; except the *Aorta*, which was much smaller, as the Figure shews.

XI. *An Account of the Preparation and Uses of the various Kinds of* Pot-ash; *by* John Mitchell *M. D. & F. R. S.*

Read Nov. 17 *and* 24. 1748.

ALTHO' Pot-ash is a thing daily used, and well known even to the Vulgar; yet, as the making it is a mechanic Art, practised only by the Vulgar, and neglected and overlooked by the Learned, so we have had

no ſatisfactory Account of it; and they, who underſtand it, generally keep it a Secret, leſt others ſhould learn ſo beneficial an Art. But as it is a Commodity that no Nation hardly can well be without, either for making Soap, Glaſs, Dying, or Bleaching, ſo the Way of making it is generally underſtood in moſt Countries, except our own. For in *France*, and other Countries where they make Wine, they make a kind of Pot-aſh in an eaſy manner from the Lees of their Wine. In thoſe and other more Southern Climes, they have many kinds of Herbs hereafter mention'd, either ſpontaneous, or cultivated on purpoſe, which they as eaſily convert into Pot-aſh. In *Germany*, and other more Northern Countries, they make great Quantities of Pot-aſh by extracting the Salts of their Wood-aſhes, in a manner that is well known. But it is only in *Ruſſia*, *Sweden*, and other Northern Nations, where the Art of converting their Wood-aſhes into Pot-aſh, without the tedious Proceſs of Elixiviation, is either well known to the Learned, or practiſed by the Vulgar.

By this means moſt Nations are ſupplied with this neceſſary Commodity of their own, except the *Engliſh*, who might be ſupplied with any Quantities of it, from the great Plenty of otherwiſe uſeleſs Wood they have in their Colonies, if not at home, if they knew how to make it. But it ſeems this Art is ſo little underſtood among us, that many Attempts I have known to make Pot-aſh have all proved unſucceſsful merely upon that account, ſo as to be intirely laid aſide. This has put me for ſome time upon inquiring into the Ways of making

this

this Commodity, of which ſeveral have been ſuggeſted to me, from the ſeveral Trials and Informations hereafter mentioned.

It is well known, that the Aſhes of all kinds of Vegetables whatever afford Pot-aſh in ſome meaſure or other; altho' ſome are much more fit for that Purpoſe than others, which may be determined from the Experiments of *Redi* in the *Philoſoph. Tranſ.* N°. 243, p. 281. *Boerhaave*, *Merret*, and others; ſo that we need not inſiſt upon them here.

As for the Trees and Herbs of our Colonies in *North America*, moſt of thoſe that are common in their Woods are known to be fit for this Purpoſe, as the Aſhes of them all, burnt promiſcuouſly in their Houſes, make a very ſtrong Lye fit for Soap. Of theſe, the fitteſt for that Purpoſe is their *Hiccory*, the moſt common Tree in their Woods, which makes the pureſt and whiteſt Aſhes, of the ſharpeſt Taſte, and ſtrongeſt Lye, of any Wood I have ſeen. Their *Stickweed* is ſaid to do the ſame; which is as common a Weed. For this Reaſon the Aſhes of both theſe Plants were uſed by our *Indians* there, inſtead of Salt, before they learnt the Uſe of common Salt from the *Europeans*. The Aſhes of *Tobacco* likewiſe, when damnified, or not fit for a Market, or its Stalks, Stems, and Suckers, of which great Quantities are thrown away, and rot and periſh, are very fit for Pot-aſh, as they contain a great deal of Salts, and are well known to make a ſtrong Lye.

On the other hand, *Pines*, *Firs*, *Saſſafras*, *Liquid Amber*, or *Sweet Gum*, or all odoriferous Woods, and thoſe that abound with a Reſin or

Gum,

Gum, are unfit for Pot-aſh, as their Aſhes are well known, even to our Planters, to make a very weak Lye, unfit for Soap.

Beſides theſe that contain little or no Salt, there are ſome other Vegetables that afford a large Quantity of it, but make a bad kind of Pot-aſh, at leaſt for many Purpoſes, on account of a neutral Salt with which they abound. This ſeems to have been the Caſe of the Pot-aſh made in *Africa*, in a Manufacture of that Commodity ſet up there by the *African* Company, which Mr. *Houſton* (who was chiefly concerned about it) tells us, in his *Travels*, proved ſo bad, on account of a neutral Salt it contained, that the Manufacture was left off on that account; or, perhaps, from their not knowing how to make it aright. What thoſe Vegetables are, that afford this kind of Aſh, is not well known, if it be not Fern, and ſome Sea-Plants.

Whatever Vegetables we make our Pot-aſh of ſhould be freſh or green, and no-ways rotten, dried, or decay'd. They ſhould likewiſe be burnt to Aſhes by a ſlow Fire, or in a cloſe Place; otherwiſe, when they are burnt in the open Air by a ſtrong Fire, great Quantity of the Aſhes is conſumed in Smoke, by the ſaline and terreſtrial Parts being carried up in Fumes, before they are ſeparated from theſe exhalable Parts by the Action of the Fire. For the Difference between burning Wood in a cloſe Place, or the open Air, is ſo great, that the Quantity of Aſhes obtained from one is more than double the other. This we learn from the Experiments of *Lundmarck* hereafter mention'd, who tells us, he burnt a Quantity of Birch in a cloſe

close Stove, from which he obtained five Pounds of Ashes; whereas the same Quantity of the same Wood burnt in the open Air, yielded only two Pounds.

It is for this Reason, that most People who make Pot-ash, burn their Wood in Kilns, or Pits dug in the Ground; altho' the *Swedes* burn it in the open Air, as the Author above-mention'd informs us. This first Step, or the burning the Wood to Ashes, seems to be taken by many for the whole Process of making Pot-ash; for they who pretend to have learned this Art in *Russia*, as well as *Lemery* and some other Authors, hardly give us any other Account of it.

But, in order to convert the Ashes, prepared in this or any other manner, to what is called Pot-ash, there are many different Ways practised in different Countries, which make as many different kinds of Pot-ash, that are all to be found in our Markets, and have all their respective Uses.

1. The first of these is commonly called Pearl-ashes by our People, who import great Quantities of it from *Germany*. This is no other than the lixivial Salt of Wood-ashes, extracted by making a strong Lye of them, and by evaporating it to Dryness, in a manner that is well known, and sufficiently explained by *Kunkelius* in his Art of making Glass, *Boerhaave*, and many others; so that we need not insist upon it here; we shall take a more fit Opportunity to explain it, for the Use of our People in *America*.

2. But the Art of converting these Wood-ashes into Pot-ash, without this tedious Process of Elixiviation, is

is only practiſed in *Ruſſia*, *Sweden*, and other Northern Countries, where it has been lately diſcloſed by one *Lundmarck*, who tells us he had often made it himſelf, in the manner he now deſcribes. This Account is contained in an academical Diſſertation upon this Subject at *Aboe* in *Sweden*, and was communicated to me by Dr. *Linneus*, Profeſſor of Botany at *Upſal*, as a genuine Account of this Art; which I think has hitherto been generally unknown.

This Author tells us, "They have many large "Woods of Beech in *Smoland*, and other Parts of "*Sweden*, in want of which they take Alder: Of "theſe they are allowed to uſe only the old and "decaying Trees for this Purpoſe, which they cut to "Pieces, and pile in a Heap, to burn them to Aſhes, "upon the Ground, by a ſlow Fire. They carefully "ſeparate theſe Aſhes from the Dirt or Coals in them, "which they call raking them; after which they col-"lect them in Baskets of Bark, to carry them to a Hut "built in the Woods for this Purpoſe. This they con-"tinue to do till they have a ſufficient Quantity of "theſe Aſhes. Then their whole Art follows; for "which they chooſe a convenient Place, and make "a Paſte of theſe Aſhes with Water, by a little at "a time, in the ſame manner, and with the ſame "Inſtruments, as Morter is commonly made of "Clay or Lime. When this is done, they lay a "Row of green Pine or Fir-Logs on the Ground, "which they plaſter over with this Paſte of Aſhes: "Over this they lay another Layer of the ſame ſtrait "Logs of Wood, tranſverſely or acroſs the others, "which they plaſter over with the Aſhes in the ſame

" manner.

" manner: Thus they continue to erect a Pile of
" these Logs of Wood, by Layer upon Layer, and
" plastering each with their Paste of Ashes, till they
" are all expended; when their Pile is often as
" high as a House. This Pile they set on fire with
" dry Wood, and burn it as vehemently as they
" can; increasing the Fire from time to time, till
" the Ashes begin to be red-hot, and run in the
" Fire. Then they overset their Pile with Poles,
" as quickly as they can; and while the Ashes are
" still hot and melting, they beat and clap them,
" with large round flexible Sticks made on pur-
" pose, so as to incrust the Logs of Wood with the
" Ashes; by which the Ashes concrete into a solid
" Mass as hard as Stone, providing the Operation has
" been rightly performed. This Operation they
" call *Walla*, i. e. *Dressing*. At last they scrape
" off the Salt thus prepared, with iron Instruments,
" and sell it for Pot-ash; which is of a bluish dark
" Colour, not unlike the *Scoriæ* of Iron, with a
" pure greenish white Salt appearing here and there
" in it."

All the Pot-ash we have from *Russia*, *Sweden*, and *Dantzick*, is exactly like what our Author here describes, and seems to be made in this manner. It is, however, generally observed, that the *Russian* is the best of these, on account of the greater Quantity of Salt in it. Now if, in the preceding Process, we make our Paste of the Ashes with Lye, instead of Water, it is plain the Pot-ash will be impregnated with more Salt, and make all the Difference there is between these Sorts of Pot-ash. This then is likely to be the Practice in *Russia*; where their

Wood may likewiſe be better for this Purpoſe, and afford more Salt. This is well known to be the Caſe of different Kinds of Wood: So our Author above-mentioned tells us, he obtained $2\frac{27}{64}$ ℔ of Salt out of eight cubic Ells of Poplar, which was very ſharp and cauſtic; but the ſame Quantity of *Birch* afforded only one Pound of Salt, and that not ſo ſtrong; and *Fir* hardly yielded any at all.

The Way of making Pot-aſh above-deſcribed may be the more eaſily underſtood by our People in *America*, for whom this is chiefly intended, as it is the ſame with their Way of making Lime of Shells, the only Lime they uſe in moſt Places. Theſe Shells they burn to Lime between the Layers of a Pile of Wood (inſtead of a Kiln) till it is reduced to Aſhes, in the ſame manner as is here directed to be done with Aſhes, to make Pot-aſh. The Lime, thus made, is reckoned very good; but, as it is impregnated with the Aſhes of the Wood, and the marine Salt that is often in the Shells, it is apt to make the Houſes that are built with it very damp in moiſt Weather; ſo that the Water often runs down their Walls in Streams; which cannot but be very unwholſome in an Air that is naturally cloſe and damp: The only Way to prevent which would be, to waſh and dry their Shells frequently, and burn them in dry Pine, that afford little or no lixivial Salt. But to return to our Purpoſe:

3. There is another Way of making Pot-aſh, practiſed chiefly in *England*; where they make it in the following manner, as I am informed by ſeveral, who have ſeen it done:

With

With their Ashes of Fern, or Wood of any kind, they make a Lye, which they reduce to what they call Pot-ash, by burning it with Straw. To do this, they place a Tub full of this Lye nigh a clean Hearth of a Chimney, in which they dip a Handful of loose Straw, so as to take up a Quantity of Lye with it. The Straw thus impregnated with Lye they carry as quick as they can, to hold it over a blazing Fire on their Hearth, which consumes their Straw to Ashes, and at the same time evaporates the Water from the Salts of the Lye. Over the Blaze of the first Parcel of Straw they burn another dipt in Lye in the same manner. This they continue to do till their Lye is all expended. By this means the Coals and Ashes of the Straw, and Salts of the Lye, are left on the Hearth, and concrete together into a hard solid Cake of a greyish black Colour, which they scrape off, and sell for Pot-ash.

This is an easy Way of making Pot-ash, in want of proper Vessels to extract the Salt of the Lye by Evaporation, or in want of Wood to reduce the Ashes to Pot-ash in the Way above-mentioned, for which it seems to be contrived, and for which it is only to be commended. For the Pot-ash made in this manner is full of the Coal of the Straw, and its Salt is not so strong, as our Workmen say, or so sharp and corrosive as the Salt of the foreign Pot-ash, that is calcined in an open Fire; besides other Differences hereafter mentioned; which makes this Pot-ash unfit for some Purposes, and not above half the Value of the foreign.

4. They have a very different Way in the North of *England* of reducing their *Kelp* to Pot-aſh, which they uſe for making Alum. This is made of the different Kinds of *Fuci*, or Sea-Weeds thrown up on the Shore, or gather'd on the Rocks; which they dry a little in the Sun, and afterwards burn them in a Kiln, built of the Stones they find on the Shore, in a cylindrical Form, and about two Foot or leſs in Diameter. In this they firſt burn a ſmall Parcel of the Herb, and before it is reduced to Aſhes they throw on more, till the Kiln is full, or their Materials are expended. This is ſaid to reduce the Aſhes to a hard and ſolid Cake, by the Heat of the Kiln, and Quantity of Salt in the Herb, which makes what is commonly called *Kelp-Aſhes*.

There are ſome other Ways of making Pot-aſh, ſuggeſted by ſeveral, both Authors and others, which appear to be more eaſy and ready than any of the above-mentioned; for which Reaſon they are apt to be tried by thoſe who make Attempts of this kind. Theſe are deduced from what they reckon the Nature and Properties of this Production: And there is no doubt, but if that was well underſtood, it might afford ſome Inſight in the Way of making it. For this Reaſon we made the following Experiments with the beſt *Ruſſia* Pot-aſh, in order to diſcover its Nature and Properties, and how they are moſt probably communicated to it; that we might ſee what we are to make; in order to imitate the beſt, or to make what is accounted good Pot-aſh.

1. *Ruſſia* Pot-aſh, as it is brought to us, is in large Lumps, as hard as a Stone, and black as a

Coal,

Coal, incrusted over with a white Salt, that appears in separate Spots here and there in it.

2. It has a strong fetid sulphureous Smell and Taste, as well as a bitter and lixivial Taste, which is rather more pungent than other common lixivial Salts.

3. A *Lixivium* of it is of a dark-green Colour, with a very fetid sulphureous Smell, and bitter sulphureous Taste, somewhat like Gunpowder, as well as sharp and pungent like a simple *Lixivium*.

4. Altho' it is as hard as a Stone, when kept in a close Place, or in large Quantities together in a Hogshead; yet, when laid in the open Air, it turns soft, and some Pieces of it run *per deliquium*; whilst most other kinds of Pot-ash only turn friable, and crumble in the open Air.

5. It readily dissolves in warm Water, but leaves a large Sediment of a blackish grey Colour like Ashes, which is in a fine soft Powder, without any Dirt or Coals in it, that are to be observed in most other kinds of Pot-ash.

6. As it is dissolving in Water, I have scummed off from some Lumps of it a dark-purple bituminous Substance, like *Petroleum* or Tar, which readily dissolved in the *Lixivium*.

7. This, or any other true Pot-ash, or a *Lixivium* made of them, will presently tinge Silver of a dark-purple Colour, difficult to rub off; whilst a mere lixivial Salt has no such Effect.

8. Pieces of this Pot-ash boiling in Water make a constant Explosion like Gunpowder; which was so strong as not only to throw the Water to some Height, but to lift up and almost overset a stone Cup

in

in which I boiled them. Theſe Exploſions were owing not ſo much to the included Air, which ſome perhaps may imagine, as to the ſulphureous Parts of the Compoſition expanding and flying off: For this boiled *Lixivium* had neither the green Colour, nor fetid ſulphureous Smell and Taſte; at leaſt in any degree like what it has when made of the ſame Pot-aſh by a ſimple Infuſion in warm Water.

9. I evaporated ſome of the green *Lixivium*, made only by Infuſion, and filtred thro' a double Rag: As ſoon as it began to boil, a green Powder, to which its Colour is owing, fell to the Bottom, and the Lye became pale. After it was evaporated to a Pellicle, and ſet in a cool Place, a Salt ſeparated from it on the Sides of the Cup, in angular Cryſtals like Tartar. Theſe Cryſtals were ſoon formed, and in pretty large Quantities, but were difficult to ſeparate from the alkaline Lye and Salt, in which and the open Air they were apt to diſſolve: But from the Pellicle I obtained ſome Pieces of the ſame Salt that would not diſſolve in the open Air.

10. Oil of Vitriol makes a ſtrong Effervеſcence with this green Precipitate, with a white Fume, and a very ſtrong ſulphureous Smell. It does the ſame with theſe white Cryſtals, altho' the ſulphureous Smell is not ſo ſtrong. But with the pure fixed Alkali there was no ſuch ſulphureous Smell to be diſcerned.

From theſe Experiments we may determine ſomething about the Nature and Contents of Pot-aſh. This we are the better enabled to do, from the accurate

accurate Experiments and Reaſonings of the learned Mr. *Geoffroy*, on a like Subſtance made of Charcoal and an Alkali Salt calcined together, in which he obſerved all the Properties and Contents of Potaſh above mentioned, particularly related in the *Memoirs* of the *Royal Academy*, for the Year 1717. This was made of the ſame Materials, and had all the Properties above-related of our Pot-aſh; particularly a green *Lixivium*, a ſtrong ſulphureous Smell and Taſte, a ſulphureous green Precipitate, cryſtallized Salts, and ſulphureous Fumes with Oil of Vitriol. From hence this learned Author concludes, that this Subſtance contained the active ſulphureous Parts of the Wood, blended with more active igneous Particles. Theſe, united with the alkaline Salts, make a kind of Soap, or ſulphureous ſaponaceous Salt, reſembling Soap of Tartar, or *Hepar Sulphuris*. The cryſtallized Salts he attributes to the Acid of the Wood, mixing with the alkaline Salts. All theſe Parts of the Wood then are contained in our Pot-aſh; and he obſerved the ſame in the common *Soda*, or *Cineres clavellati*; altho' they are in a leſs Degree in that than in the *Ruſſian* Pot-aſh.

Beſides theſe, he ſhews that Pot-aſh contains a metallic Subſtance, which affords the *Pruſſian* Blue. We may add further, that the Combination of theſe Principles makes many Properties in Pot-aſh, more than what reſult from them in a State of Separation. The moſt remarkable of theſe ſeems to be its exploſive Quality; which we take to proceed from the cryſtallized Salts approaching to the Nature of Nitre, and uniting with the Sulphur and Charcoal; by which they form, from all theſe Ingredients of

Gunpowder,

Gunpowder, a kind of that explosive Substance, whose Parts are highly rarefied in an intense and confined Heat, by which they readily explode in boiling Lye.

By this we may perceive, that the Difficulty in making Pot-ash aright, is, first, to reduce the Materials to Cinders and Ashes, and at the same time to preserve their volatile, sulphureous, and exhalable acid Parts, that are totally destroyed in such a Degree of Heat; and, secondly, to calcine these Ashes still further, so as to flux their Salts, and vitrify their terrestrial Parts, and at the same time to keep them separate from each other, or prevent their running into an indissolvable Glass. To give Pot-ash some of these Properties, seems plainly to require a Degree of Heat that will totally deprive it of others.

The most likely Way by which it comes to receive all these Properties, is from the Way of making it in *Sweden* above described. In that Process, the green Fir, in which the Ashes are burnt, impregnates them with the acid saline Parts of the Wood or Tar, which is well known to be in pretty large Quantities, and is absorbed and fixed by the alkaline Salts, and porous terresttial Parts of the Ashes in this Process; so that, besides the fixed alkaline Salts of the Ashes, the Pot-ash, thus made, must likewise contain the more volatile Salts of the Pine, which are exhaled in Smoke by burning the Pine alone in the open Air. Besides these, it likewise contains the resinous Parts and sulphureous Fumes of the Pine, that are hindered from exhaling by the Heap of the Mass.

At

At the ſame time the alkaline Salts are fluxed in the open Fire, and in a manner vitrified with the terreſtrial Parts of the Aſhes, which gives them their hard and ſolid Conſiſtence; whilſt the ſulphureous and acid Parts of the green Wood hinder them from turning to a perfect Glaſs, or inert *Calx*. All theſe Parts united together in the Fire, make that ſaponaceous Subſtance we find in the Pot-aſh thus made, which further hinders the Vitrification of the Maſs, and endows it with many of its moſt peculiar and active Properties.

From hence we may ſee how difficlut it is to make a Subſtance endowed with all theſe Properties in any other manner. This is the Reaſon why we could never before make Pot-aſh equal to that of *Ruſſia*, and the other Northern Countries, altho' we have much greater Plenty of Materials and perhaps better: For this Way of making it has never before been thought of by the Learned, or practiſed any-where elſe, as far as I can learn.

Somewhat of the ſame Qualities are communicated to the *Engliſh* Pot-aſh, by the Way of making it above-deſcribed; but in a Degree as much inferior, as dry Straw, uſed for that Purpoſe, is to green Wood: Accordingly our Workmen find that Pot-aſh as much inferior to the foreign, for many Purpoſes.

From this Account of the Contents and Qualities of Pot-aſh, and the Way of making it, we may form ſome Judgment of the other Ways of making it, propoſed by Authors, and ſuggeſted by many. Thus *Lemery* and others tell us, Pot aſh is made in *Ruſſia*, and all the Northern Countries, only by

calcining the Aſhes in Pits brick'd within, and ſprinkling them well with Lye, till they become hard and ſolid. But ſuch a Calcination of Aſhes with a lixivial Salt, muſt render them whiter, inſtead of black, and muſt further deſtroy the active ſulphureous Parts of the Wood, which we find in Pot-aſh rightly made. So that this only leaves the Aſhes in the State they were at firſt, or turns them into a kind of indiſſolvable Glaſs, as we have found upon Trial.

This, and the like Miſtakes about the Way of making Pot-aſh, ſeem to proceed from a general Error concerning the Nature of it; for it is commonly ſuppoſed to be only a kind of inert *Calx*, impregnated with nothing but a lixivial Salt. Some ſuch Miſtake ſeems to have fruſtrated all the Attempts hitherto made of making Pot-aſh in *America*; for, upon Trial, what they have made there was found to be no better than common Aſhes.

But the moſt general Miſtake about the Way of making Pot-aſh, ſeems to proceed from the Accounts we have of making it, from Glaſswort, and ſome marine Plants, which are ſaid to be eaſily converted to this kind of Subſtance, in the manner abovementioned. But we apprehend, the Way of making it from Wood muſt be very different: For theſe Herbs are eaſily reduced to Aſhes by a ſmall Fire, that does not intirely conſume their ſulphureous Parts, which Wood is not. Theſe Aſhes abound with a great Quantity of alkaline and ſome neutral Salts, that readily convert them to a hard and ſolid Conſiſtence, which Wood does not. They have likewiſe few or no terreſtrial Parts, to run them into an indiſſolvable Glaſs, when fluxed in the Fire,

as

as happens in Wood-ashes. Besides, these Herbs have few or no sulphureous or acid Parts, like most Woods; and the Pot-ash made of them has few of these Principles in it, like what is made of Wood.

It is however generally said, if we burn our Wood in a close Place, as a Kiln in which we burn Lime, or make Charcoal, or a Pit dug in the Ground, we may impregnate the Ashes with the sulphureous Fumes and acid Parts of the Wood, only by the Closeness of the Place, or by smothering the Fire in it. If at the same time we impregnate them with a greater Quantity of lixivial Salt, it will flux the whole Mass, and make it run into a solid hard Consistence like Pot-ash. This is commonly directed to be done, by throwing fresh or green Wood or Herbs upon the others, as they are burning, before they are quite reduced to Ashes; or by smothering the Fire, as in making Charcoal; and at the same time to sprinkle the Ashes, thus burnt, with a strong Lye from time to time, in the manner commonly practised with Glasswort.

This would be a more ready Way of making Pot-ash than any of the above-mentioned; but as those who give their Advice about it, have neither tried it, nor seen it done; and those who have tried this or any other Way, find more Difficulty in it, than they at first imagined, we shall suspend our Judgment about it, till we see it fairly tried, lest we should deter some from making useful Experiments of it, or lead others into fruitless and expensive Attempts.

By the various Ways of making Pot-ash above-mentioned, and the different Materials it is made of, there appear to be many different Kinds of it;

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that have as different Qualities. It would lead us too far beyond our present Design, to give a particular Account of each of these; but as they are used in many of our Manufactures, it seems worthy Inquiry, to know what Sorts are generally used, and what are the fittest to be used in them.

The Workmen in *England* make two general Kinds of it, which they distinguish by the Names of Pearl-ash and Pot-ash. The first is a mere lixivial Salt, which is supposed to be the only Ingredient of any Efficacy in Pot-ash; but, upon Trial, there is found to be a great Difference between them, especially in making Soap. The Salt is so weak in the Pearl-ash, that it does not intirely dissolve and unite with the Fat. The Reason seems to be, that these Salts are dissolved in Water, in order to extract them, by which they lose many of their caustic igneous Parts; whereas in Pot-ash, the Salts are calcined and fluxed in an open Fire, with the ignited terrestrial Parts of the Ashes, which makes them more sharp and corrosive: They are likewise incorporated with the Coal, and fuliginous Parts of the Vegetables they are made of, or with the resinous Parts of Fir, which gives them the sulphureous Quality above-mentioned, and makes a kind of *Soap* of *Tartar*, or *Hepar Sulphuris*, in all Pot-ash; which makes these Salts so ready to dissolve, and incorporate with Oil, or other pinguious Substances.

This is perhaps the Reason, why the *Cineres Russici* are ordered for this Purpose, instead of a mere lixivial Salt, by the *College* of *Physicians* in their late Dispensatory. The Soap made of them must be impregnated with their heating sulphureous Quality,

Quality, which will make it more aperient and detergent, but not ſo mild and ſoft as ſome others; by which it may be more fit for obſtinate and indurated Obſtructions, but will be more offenſive to the Stomach; which is much complained of by ſome People, who take large Quantities of the ſharper Kinds of Soap.

But, to conſider Pot-aſh as a Commodity in Trade and Manufactures, which is its chief Uſe; it appears, that the People in *England* not only have it at a dear Rate, but the worſt Sorts of it, at leaſt for moſt Purpoſes; which cannot but have a proportional Influence on their Manufactures: For it is generally of as great, and ſome Sorts of a greater Value in their Markets, than a pure lixivial Salt; notwithſtanding the ſmall Quantity of ſuch Salt in Aſhes, and the Trouble and Expence of extracting it; which ſeems to be occaſioned by their not knowing how to convert Aſhes into this Commodity; for in *Sweden*, where this Art is known, *Lundmarck* tells us, Pot-aſh is ſold for little more than a Farthing a Pound, which coſts our Workmen nigh Six-pence.

But this is not the only Inconvenience we labour under for want of this Commodity; the Sorts we are chiefly ſupplied with are perhaps the worſt of any, and unfit for many Purpoſes for which Pot-aſh is uſed. The only Pot-aſh almoſt to be met with here, comes from *Ruſſia*, *Sweden*, and *Dantzick*, or is made in *England*. Theſe are all made either of Wood or Fern-aſhes, whoſe Salts are never ſo pure and white at the beſt, as ſome others: But, by the Way of making them, and the Experiments on them above-mentioned,

they

they appear to be impregnated with Coal, Smoak, and Soot, which renders them ſtill more foul and impure, makes them of a black, brown, or green Colour, and of a peculiar ſulphureous Quality. On this account they are intirely unfit for making white Glaſs: They make a very coarſe and ſtrong kind of Soap; they are too foul, ſharp, and corroſive for bleaching, and are as unfit for dyeing, at leaſt many Colours.

It is perhaps for this Reaſon, that the Workmen here, as they ſhewed me themſelves, make all their white Glaſs with Salt-petre; which muſt not only be more coſtly, but *Neri*, *Merrett*, and others, tell us it is not ſo good, at leaſt for the better Sorts of Glaſs, as a ſharper lixivial Salt. What they uſe for dyeing I am not ſo well appriſed of: It is ſaid, they uſe the volatile Alkali of Urine; but the *French* Pot-aſh, made of the Lees of Wine, is generally allow'd to be the beſt for that Purpoſe. So likewiſe the *Alicant* Pot-aſh is reckoned much the beſt for bleaching, and making of Soap; as the *Syrian* and *Egyptian* is for making Glaſs.

Theſe purer Kinds of Pot-aſh are all made of Herbs, that grow only in the more Southern Climates, whoſe Salts are finer and whiter, and leſs acrid and corroſive than the Salts of Wood, or moſt other Vegetables; and by the Way of extracting them by Calcination in a more open Fire, they are more free of Coal, Smoak and Soot, or any other heterogeneous Mixture. On this account they are much better for the Purpoſes above-mention'd, than the coarſe and foul Kinds of Pot-aſh that our People are ſupplied with.

All

All we have of these Kinds of Pot-ash, it seems, comes only from *Spain*; for which Reason our People were obliged to petition to allow the Importation of Pot-ash from thence, during the late War; as appears by an Order of the King and Council of the 24th of *June* 1742, since they could not do without it in many Manufactures: So that it may be worth our Inquiry, to know what it is that produces so necessary a Commodity.

This Kind of Pot-ash is commonly called *Barrilha*, from an Herb of the same Name in *Spain* that produces it. The first Account we have of this *Barrilha* is from *Amatus Lusitanus*, who leaves us much in the dark about it. It is generally said in *England* to be a Plant pretty well known to the Botanists by the Name of *Ficoides Neapolitana, flore candido. Hort. Lugd. Bat.* but for what Reason I cannot say. We have as little Reason to believe with *John Bauhine* that it is what he calls *Kali vulgare:* For Mr. *de Jussieu* has shewn us, that the true *Barrilha* is a different Plant from any of these, from his own Observations of it in *Spain*, where it was cultivated; of which he has given us a particular Account, by the Name of *Kali Hispanicum, supinum, annuum, Sedi foliis brevibus. Mem. Academ. Anno* 1717. *p.* 93. or *Alicant Glasswort.*

The Pot-ash made of this Plant, he tells us, makes the best Soap, the finest Glass, and is the best for bleaching of any other; for which Reason it is much sought after in all Countries, where they value themselves for these Manufactures. But I question very much, whether our Workmen have it either pure

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and genuine, or in ſufficient Quantities for theſe Purpoſes. All the Uſe I find made of it among them, is to make hard Soap; altho' they ſay what they have of it ſpoils their ſoft Soap, by making it curdle. This is well known to be the Effects of Sea-Salt; and Mr. *de Juſſieu* and others tell us, that the true *Barrilha* is often adulterated with Sea-weeds, which contain ſuch a marine Salt; ſo that it is probably only this adulterated Sort that they have. Accordingly, all the *Barrilha* I have found here, was of a dark-brown Colour, and very foul and ponderous; whereas the true Sort is ſaid, by all who know it, to be more porous, pure, and of a bluiſh Colour. It is for this Reaſon in all Probability, that, notwithſtanding all the *Barrilha* our Workmen have at ſo dear a Rate from *Spain*, yet they can never make ſo good Soap, as what comes from thence, and ſome other Places.

The only Way then, by which we are likely to have this Commodity either pure and genuine, or in ſufficient Quantities at a reaſonable Rate, is from the Herb itſelf that produces it. Whether or not it would grow in *England* is not known, as I believe it has never been tried: But there is no doubt but it would grow very well in our Colonies in *America*, as I am certainly informed it does in the *Spaniſh* Colonies there, where they have great Plenty of it; and a Sort that is indigenous, particularly in *Peru*, which might probably be found in our Colonies, if ſought for by thoſe who knew it. But wherever it will grow in any of the *Engliſh* Dominions, there is no doubt but it would be a conſiderable Improvement, where Pot-aſh of all kinds is ſo valuable a Commodity,

Commodity, and ſo much wanted; for it grows on the ſame Ground with Corn of any kind, which it does no Harm to, as it is a ſmall annual Herb, that does not ſpread till the Corn is ripe, or off of the Ground.

There are ſome other Plants that are known to make a kind of Pot-aſh, commonly called *Rochetta*, which is ſaid to be even preferable to the *Barrilha*, eſpecially for making Glaſs. Theſe are the firſt and ſecond Kinds of *Kali*, deſcribed by *Proſper Alpinus*, in his Account of the Plants of *Egypt*. The firſt of which is the above-mention'd *Ficoides* that grows in *Italy*, and all over the *Levant*, but the other is peculiar to *Egypt*. Theſe would be fit Improvements for our Colonies in *America*, where we ſeem to want nothing more than ſome proper Production for the vaſt Tracts of Land we are poſſeſſed of there. But theſe Plants alone afford a Commodity, which *Pr. Alpinus* and *Rauwolfius* tell us they ſaw many large Ships yearly loaded with in *Egypt*, and which gives the Excellency to the Glaſs and Soap that are made at *Venice*.

It would be worth while then at leaſt to make a Trial of a Production, that is likely to improve both our Trade abroad, and our Manufactures at home. It was this that put me upon the preſent Inquiry, as an Improvement fit for our Colonies, which if I find acceptable, I ſhall hereafter conſider ſome others.

XI. *A Letter from Dr.* Laurence Garcin, *of* Neuchatel, *F. R. S. to Sir* Hans Sloane *Bart. late P.R.S. concerning the* Cyprus *of the Ancients: Done from the* French *by* W. Watſon, *F. R. S.*

S I R,

Read Nov. 24. 1748.

THE Plant in Queſtion is a Shrub, which varies conſiderably in its Size and Figure, according to the Nature and Soil of the Country where it naturally grows, as well in *Aſia* as in *Africa*, where this Plant is much uſed, both as a Medicine, and for its agreeable Odour.

Our Author has given us the true Characters of the Fructification of the *Cyprus*, after the Method of the celebrated *Linneus*.

1. Its *Calyx* is an expanded monophyllous Cup, cut into four Lobes, pointed at their Extremities, and continuing attached to the Fruit.

2. Its *Corolla* conſiſts of four oval Petals, ſomewhat pointed and ſinuous. They grow diſtant one from the other, and are placed between the Lobes of the *Calyx*.

3. It has eight erect *Stamina*, ranged two by two almoſt horizontally, and parallel to the Sides of the Petals, and ſurpaſs them in Length about half a Line. They grow from the Baſe of the Embryo at a little Diſtance one from the other, and ariſe diminiſhing in their Bulk to their Extremities. Their *Antheræ* or Summits form each of them a little kind of Purſe.

4. Its

4. Its *Pistillum* is round, and occupies the Middle of the *Calyx*. Its Style is erect, and terminated with a pointed *Stigma*. Its Length somewhat exceeds that of the *Stamina*.

5. Its *Pericarpium* is a round dry Capsule, slightly four-corner'd; each of which Corners has a small Prickle. It is divided into four Compartments by an extremely delicate Membrane, arising from a *Placenta* which occupies the Centre of the Capsule.

6. Its Seeds are small and numerous: Each of them is pyramidal, and somewhat quadrangular, of which the Point is sometimes streight and sometimes crooked. Every Seed is fastened by its Point to the *Placenta*, as to a common Centre, and their Bases are sustained by the Sides of the Capsule, all the Cavity of which is filled by them.

There is but one Species of this Shrub generally known through all the East; and this is subject to vary according to the Climate, the Season, and the Soil.

Its Names are,

1. According to different Nations.

Greek, κύπρος.
Latin *Cyprus*.
Hebrew, *Copher*.
Arabian and Persic } *Henna*.
Egyptian, *Elhanne*.
Italian, *Alchanna*.
Spanish, *Alkenna*.
The Portuguese in the Indies, } *Foula Aybana*.

Apothecaries, *Alcanna*.
The People of Malabar, } *Mail-Anschi*.
The Brachmans, *Mety*.
Malayans, *Daun Lacca*.
Javans, *Batschiar*.
Chinese, *Tsingka Hou*.
Indians, *Inne*.
At Bengal and Surat, } *Mendi*.

2. According to Authors.

Liguſtrum Dioſcoridis. Matthiol. 117.

Liguſtrum Ægyptiacum latifolium; item anguſtifolium. C. Bauh. Pin. 476.

Liguſtrum Ægyptium. Joh. Bauh. T. 1. p. 532.

Liguſtrum orientale. Park. 1447. Raii Hiſt. 1603.

Rhamnus Malabaricus, fructu racemoſo caliculato. Raii Hiſt. 1573.

Its DESCRIPTION.

The *Cyprus* grows generally as a Shrub of ten or fifteen Feet in Height, and has very much the Appearance of Privet.

Its Trunk grows ſometimes as thick as a Man's Thigh, is ſometimes ſtreight and ſometimes crooked, and produces a great Number of Branches irregularly. Its outward Bark is aſh-colour'd, and much furrow'd, and detaches itſelf from the Trunk of the Tree in long Scales or Pieces, by the Heat and Dryneſs of the Climate, as in the *Perſian* Gulf. Its inward Bark is reddiſh without, and whitiſh within. That of the Branches is ſmooth and red, like that of the Hazel-tree, and green within. Its young Branches are ſtreight, flexible, and moderately long. The Wood of the Trunk is hard and whitiſh.

Its Leaves are diſpoſed in different Orders upon the ſame Twig. Sometimes they are placed oppoſite in Pairs along the ſmall Branches, and this moſt generally croſs-wiſe; ſometimes by three and three; but then the Leaves are leſs, and this Diſpoſition generally takes place in the larger Branches; ſometimes they are alternate, but rarely, and then the Leaves are largeſt. The leaſt Branches are moſt charged with Leaves, the larger ones leaſt. All theſe

theſe Leaves are pointed at each End; the largeſt are two Inches long, and about an Inch broad in their Middle; the ſmalleſt bear half the Dimenſions of the largeſt: Their Edges are even: They are ſmooth, ſhining, and of a beautiful green Colour: Their middle Rib, which ſerves to each Leaf as a ſhort Pedicle, is terminated in their Point, but ſends out, in its Paſſage through the Leaf, alternately four or five nervous Filaments on each Side. Theſe Leaves are much like thoſe of Privet.

The Flowers grow in Bunches at the Extremities of the young Branches, and are endowed with a very agreeable and ſingular Odour. They are of a Straw-colour; but as they grow old and wither, they become of the Colour of a Citron. The *Calyx* is more pale than the *Corolla* of the Flowers. Its Petals are turned up as much if not more than thoſe ſmall Petals are which adorn the Centre of a double Roſe. The *Stamina*, which are white, tranſparent, and which grow from the Baſe of the Embryo of the Fruit, form as it were a double Croſs, by their almoſt parallel Situation and Extenſion between the Petals. The Lobes of the *Calyx*, being of the ſame Length and Form of the Petals, ſeem to give to the intire Flower an octogonal Figure. The Summits or *Antheræ* are ſmall, and of the ſame Colour as the Petals, each having a deep Furrow in its Bottom; the more theſe decay, the more yellow they grow, in the ſame manner as the Petals. The Furrow in the *Anthera*, which at firſt is of a paliſh black, grows of a deeper Hue, as the Flower fades: The *Piſtillum*, after the Flower is gone, grows larger in the *Calyx*, and becomes, when perfectly ripe, a dry, membranous, round Fruit, of about three Lines in Diameter. But before

it

it arrives to this State, it resembles very much a fleshy Berry, green on one Side, purplish, and sometimes black on the other, with very little Juice. This false Berry is the growing Capsule, the Side of which is soft, succulent, and very thick; which, in proportion as it increases, becomes thin, membranous, dry, and brittle: In becoming thus capacious and thin it gives Room to a large Number of pyramidal Seeds, very close one to another, and fastened all by their Points to a common Center, a kind of *Placenta*. When this Capsule is in its Perfection, its Outside is shining, and not unlike the Seed of Coriander in Colour. The *Pericarpium* is as it were divided into four *Loculi*, by Membranes so delicate, that they must be regarded with great Attention, to be satisfied of their Reality. The exterior Form of this Fruit sufficiently shews this Division, by its Roundness being interrupted by four slight Ribs, like those of a Melon, which shews as many Cells. The Membranes, which divide these Cells, arise from the *Placenta*, and are inserted into the Sides of the Capsule.

The Seeds, which fill all the Capsule, amount to about four or five dozen, according as they are more or less nourished; because the larger ones receiving more Nourishment, make the smaller ones abortive. They are always so pressed in their Apartments, that their pyramidal Figure is owing only to this Pressure, which arises from their reciprocal Increase. The pyramidal Points of these Seeds are crooked in some, and bent in others, according to the Direction given them in their growing. Their Colour is red or brown, and always somewhat shining.

Remarks.

Remarks.

We find, in the ancient Writers of Plants, such as *Theophrastus*, *Dioscorides*, and *Pliny*, who have all in their manner treated of Vegetables, of how much Esteem the *Cyprus* was among the Ancients. The Historian *Josephus*, and St. *Jerome* have mention'd it as a rare and precious Plant, placing it in the same Rank with the most valued Spices. The fine Smell, which its Flowers send forth in the Countries where they grow naturally, as in *Egypt*, *Syria*, *Arabia*, *Persia*, *&c.* has occasioned its Use in the earliest Time; and the same Use continues in those Countries. Its being twice mentioned in * *Solomon's Song*, is a very great Proof of its being much valued in the most ancient time. We there see it was accustomed to be cultivated even in their Vineyards. The Perfumers in old times made thereof an Oil or precious Ointment for various Uses; but principally to give their Anointings a grateful Odour, and to make supple the Limbs of the Body.

Modern Authors have given themselves great Trouble to be thoroughly satisfied of the History of this Plant. There have been great Controversies among them concerning it in endeavouring to settle its Description; but it must be confessed they have made a very small Progress in discovering to us its true Characters. How many Mistakes have the Botanists

* *Solomon's Song*, chap. i. v. 14. ch. iv. v. 13. In both these Places the *English* Translation of the Bible has it *Camphire*, instead of *Cyprus*.

stanist of the two last Centuries made, owing to the bad Descriptions of this Plant, which the Ancients have left us.

Dioscorides, who, by describing the Plants he treats of too briefly, always leaves their Characters imperfect, says (perhaps after some other Author more ancient than himself) *that the Leaves of the Plant in Question are like those of the Olive tree; that its Flowers are in Bunches, and that its Fruit is black, like that of Elder.* This was enough to make the *Latins* conjecture, that the κύπρος of this Author was the *Ligustrum* or Privet; and the more so, as the *Cyprus* was intirely unknown to them, since it only grew in *Egypt* and in *Syria*, where it was always called *Henna*, or *Alhenna*, and, by Corruption, *Alkanna*.

There is some Appearance, that, as the *Greeks* received a good Quantity of this Drug from the Isle of *Cyprus*, as a Species of Merchandize, they would chuse to call it *Cyprus*, rather than give it any other Denomination, on account of the Quantity furnished to them from the Isle of that Name. *Pliny* took it first for a kind of Privet of *Ligustrum*, which grew particularly in *Egypt*, and afterwards he thought it to be the common *Ligustrum* or *Europe*: This shews how uncertain he was as to the Plant in Question. He judged ill in comparing the Fruit of the *Cyprus* with that of the Jujube tree; but was more happy in likening the Fruit (Capsule) to that of the Coriander, as they agree in Colour, though that of the *Cyprus* was more large. *Matthiolus*, who thought himself greatly above his Cotemporaries in the Theory of Plants, asserts boldly, that our Plant was the common Privet: And

and in this he thinks himſelf juſtified, not only from the Deſcription of *Dioſcorides*, but from the Virtues attributed to the *Cyprus* by *Pliny*. He even ridicules thoſe who think that the *Liguſtrum* and *Cyprus* are different Plants. *Fuchſius*, who wrote before *Matthiolus*, had nevertheleſs Reaſon to believe them of a different *Genus*, by the Account given of the *Egyptian* Plant by *Pliny*; but he was wrong in confounding it with the *Phillyrea* of *Dioſcorides*, and in this Miſtake has been followed by *Dodonæus*.

Bellonius, who had ſeen this Plant in its Place of Growth, well knew that it was not the *Liguſtrum* or Privet: He ſaw alſo how the Commentators of the *Arabian* Authors were deceived in taking it for ſuch.

Rauwolf and *Proſper Alpinus*, who met with it in their Travels, after having obſerved it in the Places of its Growth, believed, as *Pliny* had done, that it was a kind of *Liguſtrum*, which approached very near to that of *Europe*. They have each of them given a different Figure; which made *Caſpar Bauhin* believe that there muſt be two new Species of *Liguſtrum*; but herein he was not follow'd by the ingenious Mr. *Ray*. In fact, we ought to acknowledge, by the Characters here ſet down, that our *Cyprus* is of a *Genus* truly different, and the only one of its kind.

The *Hortus Malabaricus* has given a Figure of this Plant under the Name of *Mail-Anſchi*, which repreſents the End of a large Branch ill-choſen, and ſomewhat wither'd, without doubt by the Fault of the Deſigner, who has drawn it in its natural Size; which is greater in *Malabar* than elſewhere, becauſe

 of

of the Rains which fall there in Abundance half the Year. This Shrub is less in all its Parts in *Arabia*, and to the South of *Persia*, because in those Countries it rains seldom; but, in Recompence, its Flowers have much more Smell than in *Malabar*. It must be remarked here upon this Occasion, that the Description just now given, and which contains the Size of the Parts, was made in a Garden in the *Persian* Gulf belonging to the *Dutch* Factory, and situate about a League from the Town of *Gameroon*, otherwise called *Bender-Abassi*, where there was one of these Trees carefully preserved, which was the first I saw in the *Indies*; as it was complete in all its Parts, having Flowers and Fruit; and as it appeared to me agreeable and curious, especially on account of the fine Smell of the Flowers, and as it was a new *Genus* to be established in Botany, I examined it with great Exactness, and noted its Characters, Figures, and Dimensions. I did not conceive it to be the *Cyprus*, not then knowing what it was. I asked the People of the Country the Name of this beautiful Shrub: They only called it *Henna*, and I could learn no other Name: They assured me it had no other Name, either in *Persia*, or in *Arabia*. It was on the 1st of *December* 1721. that I observed it, and described it under the Name of *Frutex Persicus, foliis Ligustri, flore et fructu racemoso, Henna vulgo dictus*. I thus characterized it, in Expectation of finding it, if it had already been described among Authors, after my Return to *Europe*. When I returned in 1730. I had the Satisfaction to find it in Mr. *Ray*'s History, by the Description which he has given of it, extracted from various Authors, in the

Chapter

Chapter of *Liguſtrum* under the *Synonyma* of *Parkinſon*, and to ſee it in the other Authors I have mention'd, eſpecially the Figure given by *Rauwolf*, which is not a bad one, and which is copied by *Cluſius*, *Dodonæus*, *Parkinſon*, and *Dalechamp*.

The Figure in the *Hortus Malabaricus* under the Name of *Mail-anſchi*, does not ſo happily repreſent our *Cyprus*, as that excellent Work generally does the Plants it treats of. The Leaves of this Plant there are half-wither'd, and not in their natural Diſpoſition. *Rauwolf*'s Figure is much nearer the Truth. The Flowers are not much better repreſented than the Leaves, in the *Hortus Malabaricus*; as, beſides other things of leſs Moment, the Authors of that Work have neglected to make the Petals appear between the Lobes of the *Calyx*, as always happens in a natural State; by which Diſpoſition the Flower appears of an octagonal Figure. *Rumphius*, who has written an Hiſtory of the Plants of *Molucca*, has given a Deſcription of this Shrub, not different from mine.

By what is here laid down of the Characters of this Plant, we plainly ſee that it differs widely from the *Oxyacantha* and *Rhamnus*; of one of which the Authors of the Notes to the *Hortus Malabaricus* ſuſpected the *Cyprus* to be a Species. This occaſion'd Mr. *Ray* to range it under the laſt, ſuppoſing its Fruit to be a Berry, which nevertheleſs it is not. This learned Author moreover could not think that the *Mail-anſchi* was the *Cyprus*, becauſe of the Difference in the Deſcriptions among Authors, and of the Imperfection of thoſe of *Rauwolf* and *Alpinus*. *Rumphius*, juſt now quoted, has ill compared the

Colour of the Leaves of *Cyprus* to thoſe of the Olive-tree.

This Shrub, ſo cheriſhed among the Eaſtern Nations, is cultivated in *Africa*, *Aſia*, and all the *Indies*; that is to ſay, from near the Equinoctial even to 35 Degrees of North Latitude; where it is much uſed, as we ſhall find by the great Commerce cauſed thereby in the *Levant*, according to the Relations of Travellers of Credit.

This Plant does not love Shade, even under the Torrid Zone, becauſe of the violent Rains there at the time of the Weſtern *Monſoon*, no more than it does in cold Countries, our Author means thoſe of the fifth Climate; but towards the Tropick, and even in *Arabia*, it grows beſt when a little ſhelter'd from the Sun. In hot and dry Countries, as in the *Perſian* Gulf, where I firſt ſaw it, it produced a great Number of Boughs and Branches very ſhort, which gave it the Appearance of White-thorn. On the contrary, towards the Equator, its Branches are further from each other, and longer, occaſioned by the Moiſture from the Rain. The Back ſplits into Scales, and detaches itſelf in Pieces from the Trunk, in thoſe Countries where it rains ſeldom; but in *Malabar*, in the Iſles of *Ceylon* and *Sunda*; the Back continues intire and united almoſt all the Year, becauſe of the Moiſture of thoſe Places.

Rauwolf remarks, that the *Turks* and *Moors* cultivate this Plant with Care, and even keep it in Pots, on account of the Smell of the Flowers, which ſomewhat reſemble Muſk. They keep theſe Pots in Winter in Chambers or Caves to preſerve the Plants from Cold.

Our

Our Author forgot to remark one Circumſtance, mentioned by *Bellonius* in the firſt Book of his Obſervations (*Chap.* 44. *apud Cluſium*), where it is ſaid, that the *Henna*, or *Alchanna*, which is our *Cyprus*, differs from Privet, becauſe the Leaves of Privet fall, and thoſe of *Cyprus* continue all the Year. But this Obſervation is of no Weight, becauſe this Difference is only apparent; and it is certain, that if our Privet was cultivated in *Egypt*, its Leaves would not fall off in Winter, becauſe it is not there ſufficiently cold.

The Uses *of* Cyprus.

Bellonius, who was the firſt of the Moderns who treated of this Shrub under the Name of *Alcanna*, and ſpoke of its Culture in *Egypt*, tells us, that the Powder of its Leaves is ſo great an Article of Commerce among the *Turks*, that they load ſeveral Veſſels from *Alexandria* for *Conſtantinople*, where the Sale of it is ſo great, that the Grand Signor's Revenue therefrom amounts yearly to 18000 Ducats. According to him, the great Conſumption of this Powder ariſes from its being uſed in beautifying the Skin and Nails, in making them red with a Decoction made therewith. The Women, he ſays, generally uſe it all over *Turkey*, to dye the Skin of thoſe Parts which are from the Navel downwards, as well as their Hands and their Hair. Their Children are ſerved in the ſame manner. They conſider this as a great Ornament; and that the Colour may hold longer, and penetrate deeper, they apply it uſually when they go out of the Baths. This

Practice

Practice of dyeing, to beautify the Body, is extended even to their Horses, of which they tinge the Mane, the Tail, and the Hoofs. They often add Alum to heighten the Colour. This Powder is sent from *Constantinople* to *Russia*. Let us now consider the other Properties of *Cyprus*.

It is not necessary here to take notice of what *Dioscorides* and *Pliny* attribute to this Plant; they may be consulted, if, at the same time, they are regarded as being very little skilled in its true Qualities. Our Author contents himself with saying, that the *Persians* and *Arabians*, who appear to have been anciently the first that used this Plant, frequently use at present not only its Flowers to perfume their Linen, their Cloaths, and their Tables, but make a greater Use of its Leaves in a Decoction, for the Cure of all Distempers of the Skin, as the Itch, Scabs, and Ring-worm, which the Air of their Country causes from its Heat, and from the Drought which often reigns there to a great Degree. These Disorders, if they are neglected to be cured as soon as possible in dry Climates, easily degenerate into the Leprosy; and it is on account of these Disorders of the Skin, that the eating of Pork is forbidden to People of every Religion in these Countries; because that Food there is known to occasion these Distempers.

All the Nations of the *East Indies* make use of it in Medicine, for the same, as well as for several other Disorders; but they particularly use the Leaves to dye their Nails; which our Author thinks they had originally from the *Arabians*. In dyeing their Nails, the *Indians* make use of the fresh Leaves,

which

which always grow in great Plenty in their Gardens, and apply them beaten upon their Nails, mixing with them ſometimes a little Lime and Juice of Citron. This Colour laſts a great while upon the Skin, on account of Sweating. A ſtrong Decoction of the Leaves in Water is ſometimes uſed to tinge their Nails, but more generally their Skin and Hair.

There is Reaſon to believe, that this pretended beautifying of the Skin, the Hair and Nails, which long Cuſtom has eſtabliſhed among the Eaſtern Nations, owes its Origin to a quite different Principle than that of beautifying. The Ancients had no other View in the Beginning, than the Prevention of pruriginous and leprous Diſorders in the Skin, to which their Climate ſubjected them, as well as to preſerve them from Vermin, as the Leaves of *Cyprus* have that Property. But as in uſing Baths with theſe Leaves therein, they dyed their Skin either red or yellow, according to the Preparation, they accuſtomed themſelves to this Colour by degrees, and afterwards regarded it as a ſalutary Embelliſhment.

Theſe Baths, which there are conſtantly employed for the Cleanlineſs and Health of the Skin, and which the Neceſſity of uſing has eſtabliſhed as a Point of Religion, and a Duty, for the better Prevention of theſe Maladies, is certainly a true Method to preſerve as well the Body as the Skin in a good State. Theſe good Effects are extended further by uſing the *Alcanna*; becauſe its Colour, paſſing in the Opinion of theſe People for a neceſſary Ornament, and a Mark of Cleanlineſs, makes the Practice of bathing better obſerved.

It

It ſeems to our Author, that theſe Remarks ſhould be communicated, as well as the Characters and Deſcription of the Plant in Queſtion, to render its Hiſtory more complete, and by theſe means to make it known; to the end that the Curious may form ſome Opinion of the great Praiſes which the Ancients have beſtowed upon this Plant. I am

SIR,

Your, &c.

Laurence Garcin.

ERRATUM.

In *Page* 541, *l.* 22, for *Art.* XI. *read* X.

Numb. 490.

PHILOSOPHICAL TRANSACTIONS.

For the Month of *December*, 1748.

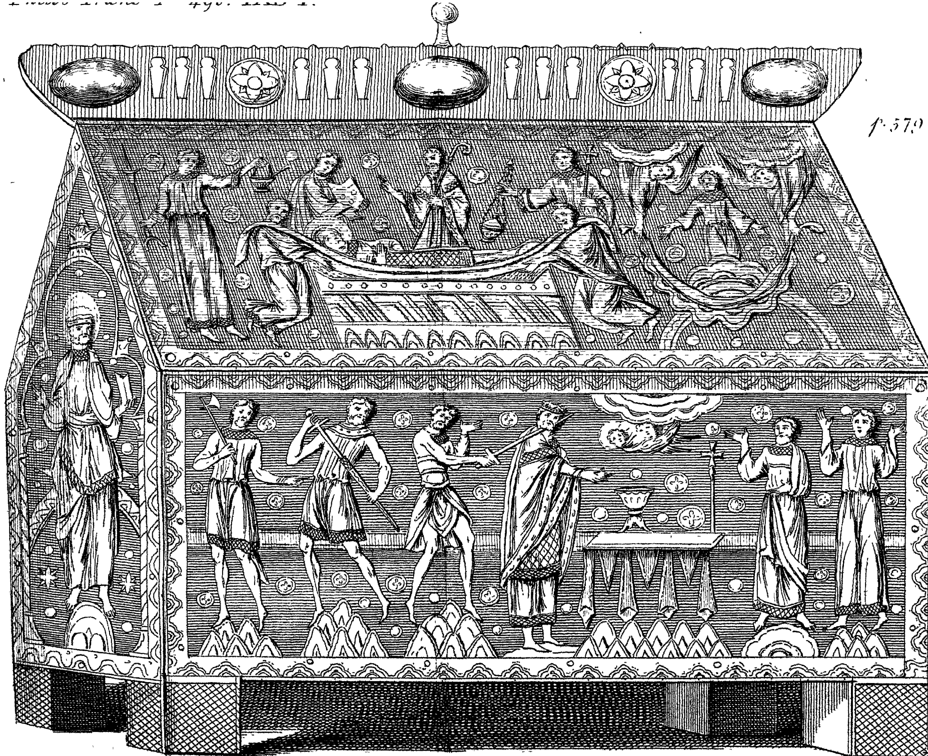
V.

The CONTENTS.

ERRATA.

N°. 486, *p.* 232, *for* Durobrovæ *read* Durobrivæ. *Ibid p.* 233, *Henry Wharton* is said to have been always *infirm and sickly:* But in his Life written by Bishop *Green*, and printed before his Sermons, it is said, *His Constitution was vigorous and healthful.*

Printed for C. DAVIS, over-against *Gray's Inn Gate* in *Holbourn*, PRINTER to the ROYAL SOCIETY, M.DCC.L.



J. Mynde sc.

A most antient Shrine, of curious enamel'd work, on Copper, in Colours.
12 inches long $10\frac{1}{2}$ high $4\frac{3}{4}$ broad.

I. *An Account of an* antient Shrine, *formerly belonging to the Abbey of* Croyland; *by* Wm. Stukely *M. D. Coll. Med. Lond. Soc. & Eccleſ. D.* Georgii *Martyr.* Lond. *Rector.*

Read Dec. 8. 1748.

THE Shrine before us is a great Curioſity. Few of this kind of Antiquities eſcap'd the general Ravage of the Diſſolution of Abbeys: For which Reaſon I thought it would be an agreeable Amuſement to the *Society* to have a View of it; and to preſerve a Drawing of it as in TAB. I. and II. The Shrine is made of Oak, plated over with Copper, upon which the Figures are chaced in Gold: The Ground is enamel'd with blue; in the Ridge along the Top are three oval Chryſtals ſet tranſparently; its Dimenſions are as expreſſed under the Print.

Mr. *Eayre* of St. *Neots* ſent it to me to have my Opinion of it. It was found in the Houſe of a Gentleman of that Neighbourhood, who never ſhew'd it during his Life-time; and who poſſibly might have given us ſome Account, as to the Hiſtory of it; and at preſent we have no means left of finding it out, but by Conjecture.

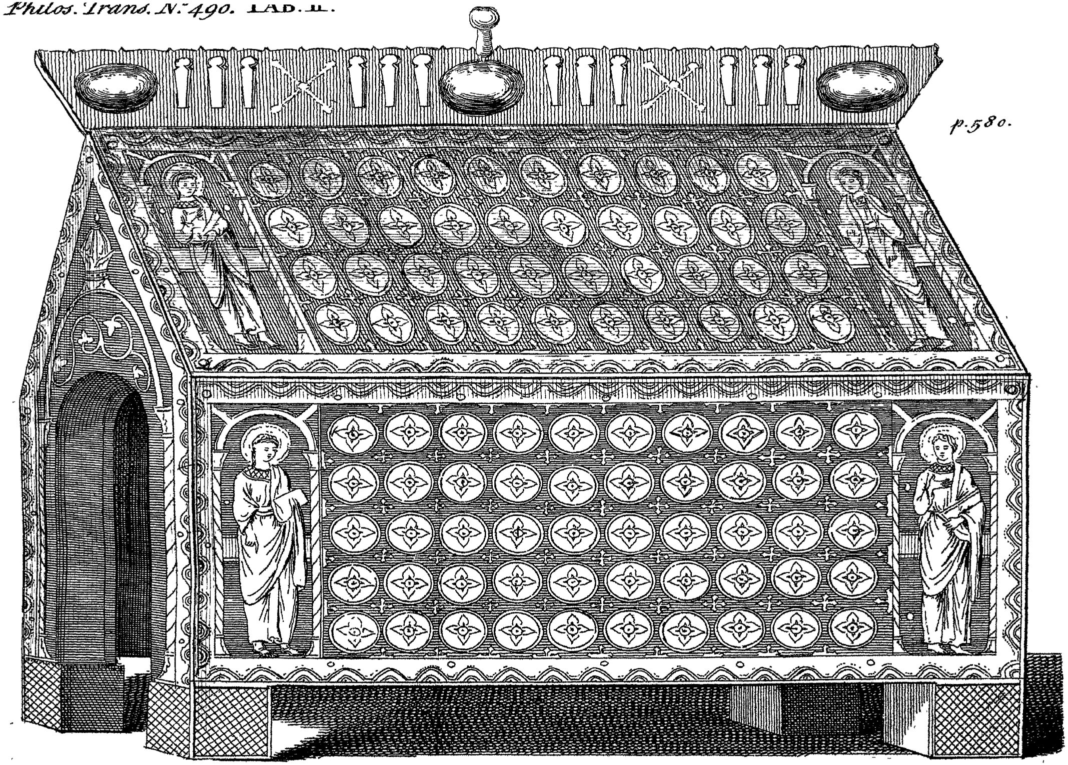
I conceive it came from *Croyland-Abbey*. There was an Intercourſe between this Abbey and St. *Neot's* Priory; inſomuch that St. *Neot's* Body was carried hence to *Croyland-Abbey*, and inſhrined there.

Theſe Shrines were made for receiving Reliques of Saints, in old Abbeys, Churches, and Cathedrals. (See a Print in the Hiſtory of *Canterbury*, and in *Dugdale's Monaſticon*, of the high Altar of the Church of St. *Auguſtin* there; no leſs than thirteen of theſe Shrines ſtanding around). Theſe were carried about in Proceſſions on their anniverſary Days; ſometimes embelliſhed with Jewels of ineſtimable Value. Beſides theſe portable ones, there were others, built of Stone, Marble, and other Materials; like that of St. *Edward* the *Confeſſor* in *Weſtminſter-Abbey*; one now in *Cheſter* Cathedral of St. *Werburga*, whereon the epiſcopal Throne is ſet, adorn'd with Sculptures of *Saxon* Kings and Saints: One of St. *Thomas de Cantelupe* Biſhop of *Hereford*, in that Cathedral. Theſe now remain. There was one in the Church of *Burton-Coggles*, *Lincolnſhire*; and of *Heckington* in the ſame County; and innumerable others, deſtroy'd at the Diſſolution of Monaſteries.

The Shrine before us, from the Manner of drawing, and Workmanſhip, I conclude to be of *Saxon* Antiquity, and that very high; now near 900 Years old. I think it gives us the Story of the Murder of the Abbot there, and his Monks, perpetrated by the barbarous *Danes*, in the Year 870.

Sept. 25. that Year, they ruſh'd into the Church of *Croyland*, whilſt the Religious were at divine Service. *Ingulphus*, Abbot of the Place, in his Hiſtory, gives us this Account. Lord *Theodore* was then Abbot of *Croyland*; who at that time pontifically officiated at the high Altar, expecting the Barbarians. King *Osketyl* cut off his Head upon the Altar. *Verus Martyr et Chriſti hoſtia immolatur*, ſays

p. 580.



The Backside of the Shrine. This elegant Antiquity is in Possession of S.r John Cotton Baronet. 1748.

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II.

ſays our Author, *Miniſtri circumſtantes omnes capitibus detruncati*: " Thus fell the true Martyr and " Lamb of Chriſt, as a Sacrifice on the Altar. All " the aſſiſtant Miniſters were beheaded likewiſe," ſays he.

The two on our Shrine are Frier *Elfget* the Deacon, and Frier *Savin* the Subdeacon.

Some Days after, when the Monks that fled returned, they found the Body of the venerable Abbot *Theodore* beheaded at the Altar.

Above is repreſented his Succeſſor Abbot *Godric*, with the Miniſters about him, putting the deceaſed Abbot into his Shroud; whilſt Angels are carrying his Soul up to Heaven.

I ſuppoſe ſome Part of this Martyr might be obtained, and kept in this Shrine.

I obſerve the famous old ſepulcral Stone in *Peterborough* Minſter-Yard, is exactly of the ſame Shape as our Shrine. It was ſet up over the Grave of the Abbot and Monks murder'd by the ſame *Danes*, the Day after thoſe of *Croyland-Abbey* ſuffer'd, *Sept.* 26. It is carv'd on the Sides with the Images of our Saviour and the Apoſtles. It is now remov'd into the Library.

Nov. 23. 1748. W. Stukely.

II. *An Eclipse of the* Sun, July 14. 1748. *observed by the Right Honourable* James *Earl of* Morton, *Mr.* le Monnier, *Royal Astronomer and Member of the* Royal Academy *of* Sciences *at* Paris, *and Mr.* Ja. Short, *Fellows of the* Royal Society.

Read Dec. 8. 1748.

THESE Obſervations were made at *Aberdour* Caſtle, belonging to the ſaid Earl, whoſe Latitude is 56° 4′ N.

Mr. *le Monnier* having come over from *France* to go to *Scotland*, to obſerve the annular Eclipſe of the Sun, *July* 14. 1748. I was deſirous to contribute all that lay in my Power to aſſiſt him, and therefore reſolved to go to *Scotland* with the Right Honourable the Earl of *Morton*, who was ſo good as to permit us the Honour of accompanying him.

We arrived at *Edinburgh July* 4. and immediately went to the College, to enquire what Preparations were made there, in conſequence of Letters we had wrote before we left *London*; when Mr. *Alexander Monro*, Profeſſor of Anatomy, informed us, that, upon Receipt of ours, he had wrote circular Letters to all his Friends in different Parts of the Country, to prepare, in the beſt manner they could, for the moſt exact Obſervation of this Eclipſe.

We found that the meridian Mark, which had been ſettled from Obſervations, by the late worthy Mr. *Mac Laurin*, was loſt, by the taking down of a Chimney, upon which it was fixed; and Mr.

Matthew

Matthew Stewart, the present Professor, having no proper Instruments, had not as yet re-established it; which we hoped to do by an Instrument, which we every Day expected from *London*; and Mr. *Stewart* having promised to make the best Observation he could, we resolved to set out for *Aberdour*, a Seat of the Earl of *Morton's*, which he readily offered to us, and did us the Honour to accompany us thither himself, having the same Desire and Curiosity to do whatever lay in his Power to contribute to an exact Observation.

Aberdour is about 10 Miles almost N. W. of *Edinburgh*. We chose this Place, as being, by the Computations of this Eclipse, at or very near the Southern Limit of the *Annulus*.

In the Castle of *Aberdour*, Lat. 56° 4′ N. and 25″ of Time West of the College of *Edinburgh*, we set up a Clock, *July* 9. and the Weather being cloudy, and our Equal-Altitude Instrument and *Transit* not being yet arrived, we on the 11th made use of an Equatorial Telescope of my Lord *Morton's*; to find corresponding Altitudes of the Sun, and at the same time put up a *Gnomon* of 15 Feet high.

Being uneasy that our Instruments were not come to Hand, and resolving to have a Communication with the College of *Edinburgh*, where they had a *Transit* Instrument; my Lord *Morton* proposed that two Cannon should be fired from the Castle of *Edinburgh*, one precisely at 12 o' Clock, and the other at 5′ after 12 on the Day of the Eclipse; and the different Observers in different Parts of the Country to be advertised of this, and to mark down the precise Time of seeing the Flash, or hearing the Sound

Sound of the Cannon; so that, after having made a geographical Map of these different Parts of the Country, and having found the exact Meridian of one Place, we should be enabled to settle the Times of all the rest by the Difference of Meridians found by this Map. This was settled and agreed to on the 12th, and an Express sent over to *Edinburgh* with a Letter from my Lord *Morton* to the Lord Justice *Clerk*, to desire this Favour of General *Bland*, who very readily granted it.

The 13th being a clear Day, we took equal Altitudes with the Equatorial Telescope, and found our Clock gained $1' \, 46''$ in two Days, and that the Sun passed the Meridian at $12^h \, 7' \, 6''$ by the Clock.

July 14th was an exceeding bad Morning both for Wind and Rain; but about 8^h in the Morning, the Clouds dispersed, and we had a very clear Sun.

In order to observe the Eclipse, my Lord *Morton* made use of a reflecting Telescope, 12 Inches focal Length, magnifying about 40 times. I made use of a reflecting Telescope 4 Feet *Focus*, magnifying about 120 times; both belonging to my Lord *Morton*. Mr. *le Monnier* made use of a refracting Telescope, about 9 Feet *Focus*, which he brought with him from *France*, armed with a Micrometer, made after the Method of Mr. *George Graham*, by the late Mr. *Sisson* at *London*.

Mr. *le Monnier* took his Station in the Garden, under the Window of the Room where the Clock was placed; my Lord *Morton* was in the Room next that where the Clock stood; and I was at the Window next the Clock.

Clock.

Clock. h ' ''	True Time. h ' ''	
8 55 0	8 47 5	The Eclipse not yet begun. Clouds come on.
8 59 13	8 51 18	Beginning of the Eclipse, found by the following Chord.
9 0 42	8 52 47	First View of the Eclipse, then considerably advanced.
9 2 30	8 54 35	Measured the Chord of the Part eclipsed; which was found equal to the Field of the great Reflector.
10 6 10	9 58 12	The illuminate Part of the Sun, measured by the Micrometer, and found $=7' 37''\frac{1}{2}$.
10 45 0	10 37 0	Again measured, and found $=7' 37''\frac{1}{2}$.
		My Lord *Morton* judged the Middle of the Eclipse, or nearest Approach to an *Annulus*, at $10^h 17' 54''$ apparent Time.
11 52 43	11 44 40	The same Phase or Chord observed as at the Beginning, and measured both in the Telescope, as at first, and by the Micrometer, and found $= 8' 25''$ of a great Circle, as verified by a Base after the Eclipse was over, which gives the End as exact as the Beginning.

Clock.	True Time.	
h ′ ″	h ′ ″	
11 56 21	11 48 18	End of the Eclipſe by the preceding Chord.

Mr. *le Monnier* meaſur'd with the Micrometer the apparent equatorial Diameter of the Moon, when ſhe was upon the Sun; which he found $= 29' 47'' \frac{1}{2}$. He meaſured alſo the apparent vertical Diameter of the Sun at Noon; which he found $= 31' 40''$.

The Micrometer, with which he meaſured theſe Diameters, was afterwards verified, by a Baſe of 2570 Feet, and two Marks, placed at right Angles to its Extremity, at the Diſtance of 22 Feet from one another.

The Flaſh of the firſt Cannon fired from the Caſtle was ſeen at $12^h\ 3'\ 4''$ by the Clock; and the Flaſh of the ſecond Cannon alſo by the Clock at $12^h\ 8'\ 4''$. The Eclipſe was ſo nearly annular, that, at the neareſt Approach, the Cuſps ſeemed to want about $\frac{1}{7}$ of the Moon's Circumference to be joined; yet a brown Light was plainly obſerved, both by my Lord *Morton* and myſelf, to proceed or ſtretch along the Circumference of the Moon, from each of the Cuſps, about $\frac{1}{3}$ of the whole Diſtance of the Cuſps from each Cuſp; and there remained about $\frac{1}{3}$ of the whole Diſtance of the Cuſps not enlightned by this brown Light; ſo that we were for ſome time in Suſpenſe whether or not we were to have the Eclipſe annular with us. I obſerved, at the Extremity of this brown Light, which came from the Weſtern Cuſp, a larger Quantity of Light, than in any other Place, which at firſt ſurprized me; but

afterwards

afterwards I imagined it muſt have proceeded from ſome Cavity or Valley made by two adjoining Mountains on the Edge or Limb of the Moon; I had often formerly obſerved Mountains on the Circumference of the Moon, more or leſs every-where round it, but never ſaw them ſo plain as during the Time of this Eclipſe; for we had the Air exceeding clear, and free of all Agitation, notwithſtanding it blew a perfect Hurricane of Wind, which began about the Middle of the Eclipſe; and I remember, in the annular Eclipſe of the Sun in the Year 1737. it did the ſame. The mountainous Inequalities on the Southern Limb of the Moon were particularly remarkable; in ſome Parts Mountains and Valleys alternately; others extended a conſiderable Way along the Circumference, and ended almoſt perpendicularly like a Precipice. My Lord *Morton* was able to ſee them very eaſily thro' his ſmall Reflector.

A little after the Middle of the Eclipſe, ſome Clouds, that ſeemed ſtationary below the Sun, appeared tinged on their upper Extremities with all the Colours of the Rainbow.

During the greateſt Darkneſs, ſome People, who were in the Garden adjoining to the Caſtle, ſaw a Star to the Eaſt of the Sun; which, when they afterwards told us, and pointed to the Place where they had ſeen it, we found muſt have been the Planet *Venus*. This Star, we were afterwards told, was ſeen alſo at *Edinburgh*, and other Places, by a great Number of People; but I did not hear of any other Stars being ſeen. The Darkneſs was not great, but the Sky appeared of a faint languid Colour. What is

pretty remarkable, is, Mr. *le Monnier* aſſured us, that when he looked at the Sun with his naked Eyes during the Middle of the Eclipſe, he could obſerve nothing upon the Sun, but ſaw the Sun full, tho' faint in his Light. This, I am apt to imagine, may be owing to his being ſhort-ſighted.

I obſerved alſo, about the Middle of the Eclipſe, a remarkable large Spot of Light, of an irregular Figure, and of a conſiderable Brightneſs, about 7′ or 8′ within the Limb of the Moon next the Weſtern Cuſp. I thought I loſt this Light ſeveral times; but whether this was owing to my ſhutting my Eyes, in order to relieve them, or not, I cannot tell. I am told, that the Rev. Mr. *Irwin* at *Elgin* obſerved the ſame. When I firſt perceived it, I called to my Lord *Morton*, who was in the next Room, but he could not ſee it.

Before the Eclipſe began, and during the whole Time of the Eclipſe, the Air, as I ſaid before, being exceeding clear, I ſaw thro' the four Foot Reflector, the Surface of the Sun cover'd with ſomething which I had never obſerved before; it ſeemed to be all irregularly overſpread with Light, and a faint Shade, eſpecially towards his equatorial Diameter. This Appearance was ſo odd, that it is difficult to deſcribe it, ſo as to give an adequate Idea of what I ſaw; but if I may be allowed the Expreſſion, it ſeemed as it were curdled with a bright and more duſky Light or Colour. This Appearance was permanent, and regularly the ſame; and if in any degree ſeen before, may have given Riſe to *Faculæ* having been ſeen in the Sun; but to me the whole Sun's Body ſeemed to be more or leſs cover'd with it.

I looked

I looked with all the Attention possible, to see if I could observe the Body or Limb of the Moon before she touched the Sun, and also after she left it, and was intirely off the Sun, but could see nothing at all of any such Appearance. I mention it to satisfy Mr. *De Lisle*, who publicly desired this might be attended to.

The Barometer had been falling for several Days before the Eclipse; and even that Morning; when it was at 29.2 Inches. But during the Eclipse it began to rise.

				Divisions.
July 11. at 8^h in the Morning the Thermometer stood at	h	′		54
at	12	0	or Noon at	56
at	4	0	*p. m.* at	60
July 12. at	11	0	*a. m.* it stood at	57
at	12	0	or Noon, at	58
July 13. at	8	30	*a. m.* it stood at	$55\frac{3}{4}$
at	1	0	*p. m.* at	$57\frac{1}{2}$
July 14. at	8	0	*a. m.* at	56
at	8	53	at	57
at	9	7	at	$57\frac{1}{2}$
at	9	20	at	$57\frac{1}{4}$
at	10	8	at	57
at	10	26	at	$56\frac{3}{4}$

All these Observations of the Thermometer were taken when it stood in the Shade; and the Times are by the Clock. Immediately after the Middle of the Eclipse, the Thermometer, when exposed to the Sun for the Space of 10′ of Time, rose only half a Division.

		Divisions
Thermometer still exposed to the Sun,		
at 10^h 46′ 00″, stood at	- - -	$58\frac{1}{2}$
at 10 51 30 at	- - -	62
at 10 57 30 at	- - -	$63\frac{1}{2}$
at 11 4 00 at	- - - -	66
at 11 10 00 at	- - -	70
at 11 34 00 at	- - - -	$75\frac{1}{4}$

Thermometer replaced in the Shade after this last Observation,		
at 12^h 54′ stood at	- -	$60\frac{1}{2}$
at 1 28 at	- - - -	61 +
at 5 50 at	- - - -	59
at 7 30 at	- - - - -	$58\frac{1}{2}$
July 15. Thermometer at 8^h *a. m.* stood at	-	56
at 9 at	- -	57
at 10 at	- -	60

These Observations were made with a Thermometer of *Fahrenheit's* Scale, the Divisions of which were very sensible. We did not at all perceive or feel any greater Degree of Cold, during the Eclipse, than we felt before it began.

The Weather being very bad at *Edinburgh*, Mr. *Matthew Stewart*, the Professor of Mathematics, could make no Observations of the Eclipse; he only saw the End at 11^h 50′ 34″ true Time; and even then the Sun was somewhat cloudy: He took however the Sun's *Transit* over the Meridian (as then supposed) at 12^h 7′ 42″ by his Clock, and heard the second Cannon fired from the Castle at 12^h 4′ 48″ by the Clock. We afterwards, in a few Days, examined his meridian Mark with a very exact equal Altitude Instrument by three

three ſeveral correſpondent Obſervations; and found his Mark 3′ 22″ of Time to the Weſt of the true Meridian. The College is about 2500 Feet diſtant from the Caſtle Eaſtward.

The Rev. Mr. *Bryce*, at *Aldiſton*, about 6 Miles to the Weſt of *Edinburgh*, Lat. 55° 55′½ N. obſerved with a reflecting Teleſcope, 9 Inches *Focus*,

	h	′	″
The Beginning of the Eclipſe at .	8	52	30
Upper Horn or Cuſp vertical, at .	9	5	0
Hitherto the Weſtern Cuſp lower than the Eaſtern.			
The two Cuſps horizontal at . .	10	13	10
The Weſtern Cuſp aſcends very faſt at	10	14	10
The Weſtern Cuſp vertical at . .	10	16	15
The Cuſp which was juſt now vertical, now becomes Eaſt, and about 30° from the Zenith to the Eaſt at	10	17	10
The Middle of the Eclipſe as near as he could judge at . .	10	17	40
The lower Cuſp at the *Nadir*, and very ragged and uneven, at . .	10	24	45
The ſame Cuſp ſtill in the ſame Poſition at . . .	10	32	5
The ſame Cuſp ſeems to begin to move towards the Weſt at . .	10	43	35
The Motion of this Cuſp ſcarce ſenſible at	10	55	45
The other Cuſp Middle between the *Zenith* and the *Nadir* towards the Eaſt at	11	0	25
End of the Eclipſe, the Sun being quite clear at	11	48	40

I shall set down the following Observations of this Eclipse just as they came to my Hand when in *Scotland*, without making any other Remark, than that, from the Disagreement among themselves, they do not all of them seem to have been made with due Accuracy and Attention; for want, I suppose, of sufficient Practice in this kind of Observations.

William Crow Esquire, at his House of *Netherbyres* near *Haymouth*, Lat. 55° 51′ N. says,

	h	′	″
The Eclipse began at . . .	8	55	0
Half of the Sun eclipsed at . . .	9	50	0
Middle of the Eclipse, $\frac{1}{6}$ of the Sun's Limb cover'd by the Moon at . . .	10	25	0
End of the Eclipse at . .	11	55	0

Mr. *John Mair*, at *Air*, Lat. 55° 30′ N. says, the Eclipse began at 8^h 45′; but that, by reason of Clouds, he could make no other particular Observation; only that, by a View he had of the Sun some little Time before the End, he thinks the End of the Eclipse might be about 11^h 48′.

Mr. *Mark*, Teacher of the Mathematics at *Dundee*, Lat. 56° 25′ N. observed,

	h	′	″
The Beginning of the annular Appearance at	10	16	44
End of the annular Appearance at . .	10	23	8

He says, the best Observations make the *Annulus* a small Matter narrower on the upper than lower Side; by which it appears the Centre of the Eclipse was to the Northward of *Dundee*.

Mr.

Mr. *John Stewart*, Professor of Mathematics at *Aberdeen*, writes, that, by an Observation made at *Monross*, Lat. 56° 41′,

	h	′	″
The annular Appearance began at . . .	10	20	0
Annulus ended at . . .	10	24	30
End of the Eclipse at . .	11	52	45

And that, by an Observation made at a Place about 18 Miles S. W. of *Aberdeen*,

	h	′	″
The Eclipse began at . . .	8	52	0
Middle at	10	21	0
End at	11	52	0

And that at *Aberdeen*, Lat. 57° 11′ N.

	h	′	″
The Eclipse began at . . .	8	55	33
Middle of the Eclipse, and annular Appearance, as near as he could judge, at	10	23	3
End of the annular Appearance at .	10	24	48

He writes also, that he received an Account from Mr. *Reid*, Minister at *New Macchar*, about 7 Miles N. W. of *Aberdeen*, who observed

	h	′	″
The Beginning of the annular Appearance at	10	18	28
And the End of the Eclipse at . .	11	49	3

Mr. *Stewart* says, that, by comparing his Observation at *Aberdeen* with this of Mr. *Reid's*, he apprehends he is in a Mistake as to his judging of the Middle of the Eclipse, and annular Appearance; and reckons, that the annular Appearance began at *Aberdeen* at 10ʰ 19′, and ended as above. By which the total Duration of the *Annulus* was 5′ 48″; and the End of the Eclipse at *Aberdeen* was at 11ʰ 49′ 33″.

The

The Rev. Mr. *Irwin*, at *Elgin*, Lat. 57° 34′, ſays, the Eaſtern Limb of the Moon touched or entered on the Weſtern Limb of the Sun at $8^h\ 57'$; tho' he ſuſpects it began a little ſooner (another having taken the Teleſcope out of his Hand); for when he looked, the Moon was a little advanced on the Diſc of the Sun about 30° from the *Zenith* of the Sun towards the Weſt.

	h	′	″
The Eaſtern Cuſp in the *Zenith* of the Sun at	9	6	10
Eaſtern Limb of the Moon reached the Centre of the Sun at . .	9	39	0
The *Annulus* began about 30° from the *Zenith* of the Sun Weſtward at	10	20	0
The *Annulus* appeared moſt perfect at	10	22	45

Tho', as nearly as he could diſcern, he thought it a little narrower on the South-weſt Limb of the Sun, than it was on the oppoſite Side. From hence it ſhould appear, that the Centre of the Eclipſe was to the Southward of *Elgin*.

The *Annulus* was obſerved to break on the South-eaſt Limb of the Sun, about 30° from the *Nadir*, at $10^h\ 25'\ 30''$.

Before the joining of the Cuſps of the Sun, as alſo at the breaking of the *Annulus*, he ſays, he obſerved a quick tremulous Motion, and ſeveral irregular bright Spots between the Cuſps, which diſappeared in a few Moments; and he thought the Moon's Body paſſed quicker about the Time of the *Annulus* (eſpecially as it was forming), than at any other Time during the Eclipſe.

Before the Weſtern Limb of the Moon reached the Centre of the Sun's Diſc, the Sun was hid under

a

a Cloud, and continued so, till within some little Time of the End of the Eclipse, which happened at 11^h 50′.

There was no Cloud all the Time of the Formation of the *Annulus*, or the Duration of it; and he thinks he is pretty right, as to the Time of its Continuance; for both the Formation and Breaking were very sensibly to be observed, and passed in a Moment; affording a very pleasant Sight, by the irregular tremulous Spots of the Sun.

He says, the Darkness, during the *Annulus*, was not so great as a little before and after; and, when greatest, was only somewhat duskish, but observable. Some saw a Star to the East of the Sun; but he saw it not, nor any present with him. He was told of it after his Observation was over.

He says, that, by an Observation taken of the Sun that Day at Noon, he found that his Clock was somewhat less than a Minute faster than the Sun. He says also, that he observed this Eclipse with a Telescope 3 Feet long, and that he had a very good Burning-glass; but that it had little Force, during the *Annulus*, and some short time before and after.

Mr. *Duncan Frazer* writes to Mr. *Monro*, Professor of Anatomy at *Edinburgh*, that he went to the House of *Culloden*, Lat. 57° 29′ N. on purpose to observe the Eclipse; it having been said, that the Centre of the Eclipse would pass there; and after having adjusted his Clock by the Regulator-Clock of a Watch-maker at *Inverness*, he observed the Eclipse with a Telescope five Feet long, and found

	h	′	″
The Beginning precisely at . .	8	37	36
Beginning of the *Annulus* at . .	10	0	10
End of the *Annulus* at . . .	10	5	10
End of the Eclipse at . . .	11	29	30

By comparing his Observation with that sent him by Mr. *Irwine* at *Elgin*, he imagines his Clock was not set to true Time, since there is so great a Difference, and more than the Difference of Longitude between the two Places will allow; it being no more than 26 computed Miles, and nearly in the same Parallel of Latitude.

Mr. *Murdock Mackenzie* (who has for some Years past been making a Survey of the Islands of *Orkney*, and whose Abilities for such an Undertaking give us Hopes he will for the future free Navigators of a great many melancholy Disasters, which formerly happened in those Seas, thro' the Want of true Charts) made the following Observation at *Kirkwall* in the Island of *Pomona* in *Orkney*, the Latitude of which is 58° 58′ N.

	h	′
Beginning of the Eclipse about . . .	8	40
End of the Eclipse about . .	11	37

He says, that, by reason of Clouds, he could not be perfectly exact, as to the precise Time of Beginning or Ending; but adds, that the Beginning cannot be more than 4′ wrong, nor the End more than 2′. He says, he is sure he did not see it annular, but that there remained about $\frac{1}{4}$ or $\frac{1}{5}$ of the Sun's Circumference intercepted at the Middle of the Eclipse.

P. S.

P. S. It having been an Opinion pretty generally received, that the darker Parts of the Moon's Surface are Water, I take this Opportunity to remark, that though those less lucid Spaces are for the most part, to Appearance, evenly extended Surfaces, when Telescopes of small magnifying Powers are made use of, yet, when they are examined with larger Magnifiers, it is easy to discern on them many Protuberances in a longitudinal Direction; and that these Risings are really elevated above the common plane Surface, is past all Question, from their projecting Shadows, always opposite to the Sun: Moreover they are of the very same Colour as the Plane they arise from, of the like smooth Surfaces, without any sensible Asperities; and invariably the same, under the like Positions of the Sun to the Moon, at least as far as I have been able to discover in 12 or 15 Years frequent Observations of them.

Ja. Short.

III. *A Letter from Mr.* David Erskin Baker *to* Martin Folkes *Esq*; *Pr. R. S. containing Considerations on two extraordinary* Belemnitæ.

S I R,

Read Nov. 24. 1748.

VARIOUS have been the Opinions of Authors concerning the Origin of the *Belemnitæ*, and as various the Syſtems and Hypotheſes advanced by them in Support of their Opinions; ſome having imagined them vegetable Productions; others have taken them for the different Parts of Animals, as Teeth, Horns, Bones, *&c.* in which even theſe again have differed, as to the referring them to land or marine Animals; and they have been by others ſuppoſed of mineral Origin, or *Lapides ſui generis*. What they really are, will, I doubt, be ſtill very difficult to determine; but, as one principal Objection to their being originally marine Bodies (which Suppoſition ſeems to carry the greateſt Colour of Probability) has been, that no marine Bodies have been found adhering to them, that Objection will be obviated by no leſs than two Specimens, from the ſame Place, of *Belemnitæ*, whereto undoubted marine Subſtances are found firmly affixed; by which Inſtances, as ſome further Light may be thrown on this Subject, that Conſideration will, I hope, ſtand as an Excuſe for my troubling you with this Paper.

Theſe curious Foſſils, which, together with the Drawings of them, I humbly ſubmit to your Examination,

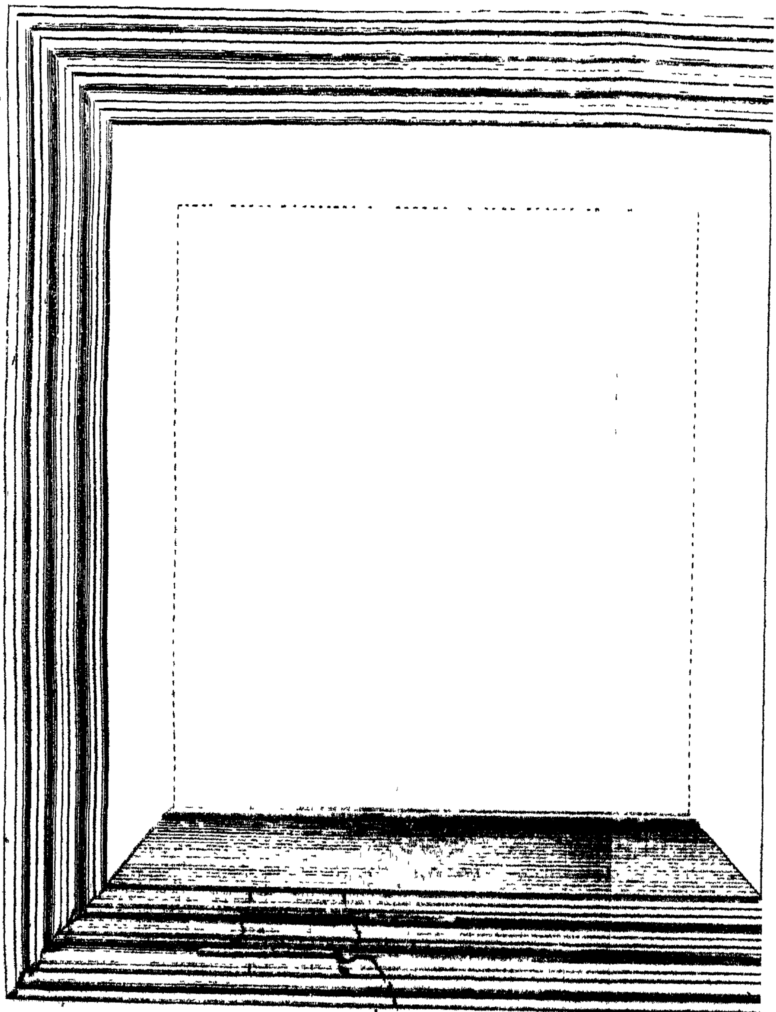


Fig. 4. p. 609.

Fig 1
p. 599.

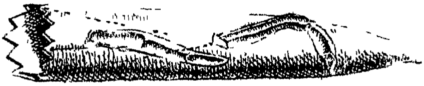


Fig 5

p 612.

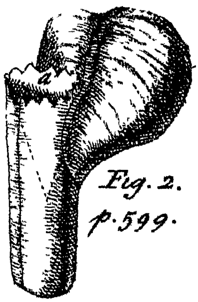


Fig. 2.
p. 599.

Fig. 3. b
p. 599.

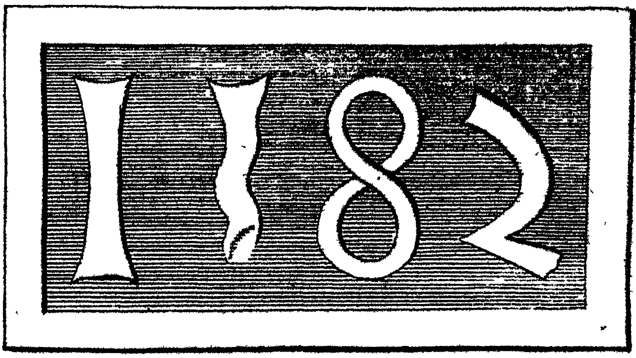


Fig. 6. p. 613.

J Mynde sc.

mination, were found in a Chalk-Pit in *Norfolk*, from whence they were ſent not long ſince to my Father Mr. *Henry Baker* F. R. S.

See TAB. IV.

Fig. 1. Is a *Belemnites*, whoſe *Apex* is perfect; the conic Cavity, and the longitudinal Seam, evidently diſtinguiſhable; which, as well as the Contexture of the Subſtance whereof it is compoſed, ſhew it to be a true *Belemnites*; but on its Surface are placed, in their natural Condition, by which I mean not at all ſeemingly petrify'd, or otherwiſe alter'd, two of thoſe *Vermiculi* that are ſo frequently found ſticking to Oyſters, Scallops, and many other kinds of Shells, when taken out of the Sea.

Fig. 2. A *Fruſtum* of another *Belemnites*, the *Apex* whereof is broken, but the conic Cavity is ſtill remaining, and ſhewn at *a*. To this *Belemnites* adheres a Shell of the Oyſter-kind, which is faſten'd thereto ſo ſtrongly, that they are not to be ſeparated without breaking: Which Shell, as well as the before-mention'd *Vermiculi*, ſeems not altered in its Subſtance, but appears like a recent one, of which many are to be met with in the Cabinets of the Curious.

Fig. 3. Shews the other Side of the ſaid Shell, wherein the *Cardo* or Hinge at *b* is plainly diſcernible; at *c* appears the broken End of the *Belemnites*, where the radiated Contexture (well known to belong to their Bodies) is repreſented, as alſo the longitudinal Seam at *d*.

As

As theſe Specimens are undeniable Proofs of marine Bodies adhering to *Belemnitæ*, ſeveral of the Curious who have ſeen them, are of Opinion, that they tend likewiſe to prove the *Belemnitæ* to be marine Productions. It may probably be objected, that theſe Shells might have been brought and depoſited near the *Belemnitæ* whereto they are affixed, by whatever mighty Change it came to paſs that Productions of the Sea are diſcover'd in moſt Countries at great Depths in the Earth, and in the Bowels of Mountains at great Diſtances from the Sea (even ſuppoſing the *Belemnitæ* to be *Lapides ſui generis*, and produced in the Earth) and that theſe Shells might be cemented to them afterwards by ſome mineral, ſtony, or other Matter. But the following Obſervations will render this improbable; for,

1. The *Vermiculi* of *Fig*. 1. are not any Species of the *Tubuli marini*, found ſometimes recent, and ſometimes foſſil, detach'd intirely from every other Body; but are of that ſort, which is perhaps never ſeen ſeparate, or in any other Manner, when recent, than attach'd and faſten'd to other Shells or Stones; and they are placed on this *Belemnites* exactly in the ſame Manner as they are commonly found on other marine Bodies; *viz*. lying on their broadeſt Side, with their Ridge upwards, and glued as it were thereto by a ſhelly Subſtance.

2. In *Fig*. 3. at *e*, is plainly to be diſtinguiſhed, that the Shell has been faſhion'd thus by the convex Surface of the *Belemnites*, in the ſame manner as theſe Shells commonly receive a Form from whatever Subſtance they adhere to; which plainly implies, that this Shell was faſten'd to the *Belemnites* when itſelf

itſelf was very ſmall, and in a growing State; and that the Shell in its Growth was formed according to the Figure of the Body on which it was affixed: But ſuch Growth could not poſſibly have proceeded any-where but in the Sea; and therefore theſe two Bodies muſt neceſſarily have been in the Sea at one and the ſame time.

There is now but one Way more, whereby theſe Shells (ſuppoſing the *Belemnitæ* to be Stones *ſui generis*) could poſſibly become affixed to them; which is, that the *Belemnitæ* might have been by ſome Accident thrown on the Sea-ſhore; and that there the Shells might faſten themſelves to them, as well as to any other Stone. But as this muſt imply ſome former Convulſion in Nature, whereby they were caſt out of their natural Beds upon the Sea-ſhore; and again a ſecond Convulſion to carry them to the Chalk-pit where they were found; ſo far-fetch'd an Objection will, I believe, carry but little Weight.

To conclude, I ſubmit to your Opinion, whether the Sides of the conic Cavity, whereto the Oyſter-ſhell is affix'd, has moſt the Appearance of a Stone or of a Shell; and remain, with the utmoſt Reſpect,

S I R,

Strand, Dec. 15.
1748.

Your moſt obedient and

obliged humble Servant,

D. E. Baker.

IV.

IV. *A Letter from* Benj. Cooke *F. R. S. to* Peter Collinſon *F. R. S. concerning a* mixed Breed *of* Apples, *from the Mixture of the* Farina.

Dear Couſin, *Newport*, Dec. 4. 1748.

Read Dec. 22. 1748.

I SENT you laſt Year a Specimen of the Effect of the *Farina* of a rough-coat Apple ſtriking on the Flower of a ſmooth-coat; I have now ſent an Example of the *Farina* of the latter changing the former into its own Dreſs and Likeneſs.

The Situation of the Ruſſeting was ſuch, that he was ſurrounded by Winter Pippins, Pearmains, and ſuch-like; and we put the Maſter-Fruit together with ſeveral of the Changelings, as they grew on the ſame Branches mixed together.

This Inſtance will ſhew what Alterations may be expected in cognate Species; and I ſhould have given an Example of a kind of Antipathy betwixt the Pear and the Apple in like Circumſtances, but was diſappointed. I am,

Dear Couſin,

Yours, moſt obliged and affectionate,

B. Cooke.

V. *A* Description *of the town of* Silchester *in its present state. With a short* Account *of an antient* Date *in Arabian figures at* Walling *near* Aldermarston *in* Berkshire. *By* John Ward, *F. R. S. and P. R. G.*

Read Dec. 22. 1748.

IN a former paper, which I had the honour to lay before this Society (1), I attempted to explain a Roman inscription cut in a stone, then lately found at *Silchester* in *Hampshire*; by which it not only appeared, that this town was the antient *Vindomis*, but likewise that it was situated within the limits of the *Segontiaci*: as to both which circumstances our best antiquaries have been at an uncertainty, and differed in their sentiments concerning them. I took notice likewise at the same time, that the traces of the antient town are yet, as I had been informed, often visible in the summer; and that the ruins of an amphitheatre still remain without the wall. But being since in that country, I had an opportunity of visiting the place myself, and geting a more perfect account of it than I expected, by the assistance of two persons in the neighbourhood, the late Mr. *John Wright* junior, and Mr. *John Stair* junior, who were both well acquainted with it, and accompanied me thither.

The

(1) *Phil. Transf.* Num. 474.

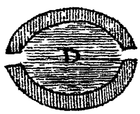
The former, who was an experienced ſurveyor, meaſured the whole circuit of the wall, with the hight of it in ſeveral places, as alſo the dimenſions of the amphitheatre, while we were on the ſpot. And the other traced out the ſeveral ſtreets, and other parts of the town, to a conſiderable exactneſs. But as only a rough draught of the plan could then be taken, I left it with them to reviſe, and tranſmit to me an accurate copy; which has been ſince done, and now accompanies this deſcription (*a*).

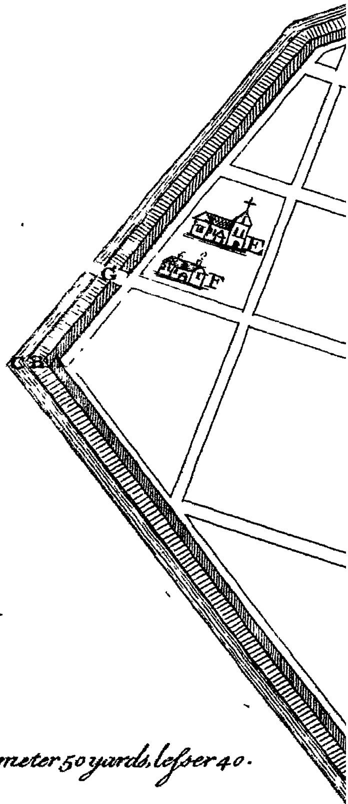
The circuit of the wall on the outſide, as therein given by the ſcale, contains near one *Engliſh* mile and a half; and the ſeveral parcels of land contained within it amount together to an hundred acres, or upwards. Indeed *Leland* ſais, that the compaſs of the wall is *about two miles, and conteyneth* 80 *acres* (1). And *Camden* ſais the ſame, except that he calls them *Italian miles* (2). But neither of them acquaint us, from whom they had their meaſurement. The wall conſiſts of nine ſides, but very unequal; which might perhaps be occaſioned by the different ſituation of the ground, which in ſome parts is uneven.

The materials, that compoſe the wall, are large flints, and rough ſtones of different ſorts, cemented together with very ſtrong mortar. And as to the manner of building it, the foundation is generally made of a row or two of ſtones laid flatwiſe; and over

(1) *Itinerary*, vol. vi. p. 48, edit. 1744.
(2) *Britann.* p. 196, edit. 1607.
(*a*) *See* TAB. III.

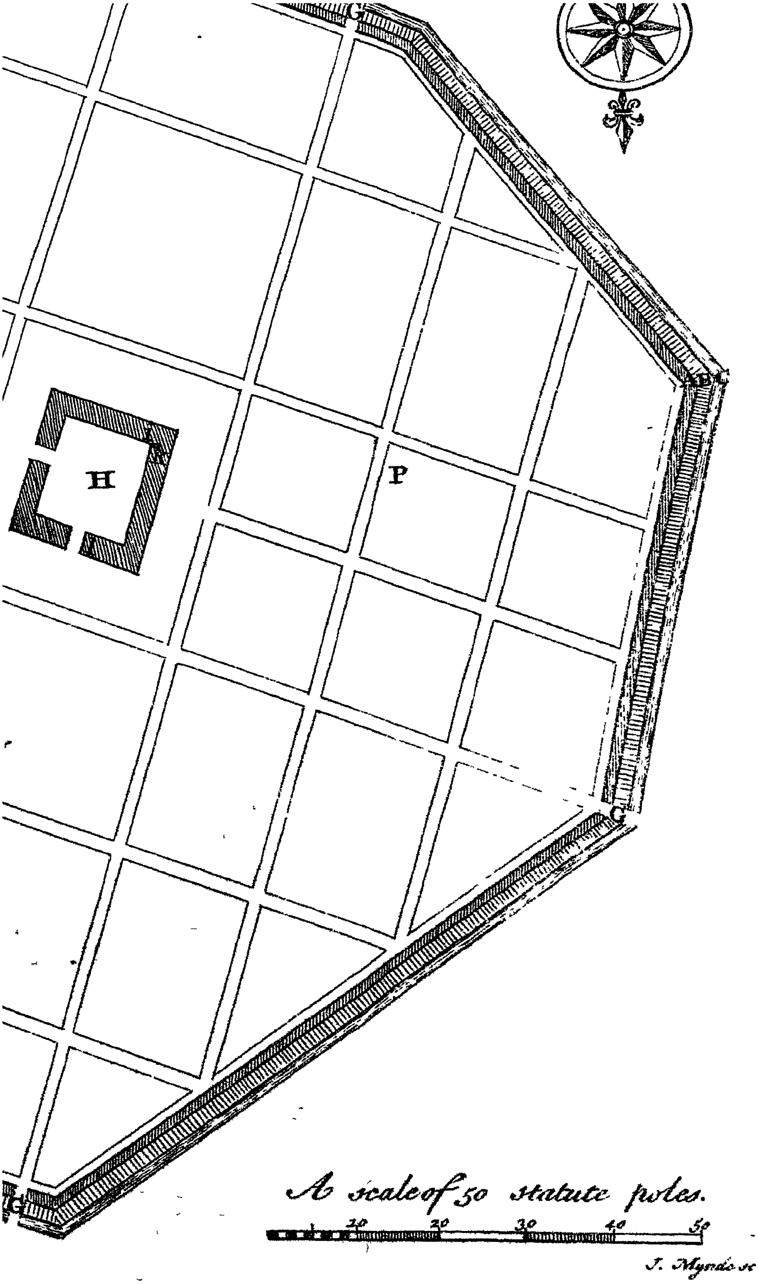
A PLAN of the ancient city of SILCHESTER in Hampshire, taken by IOHN WRIGHT Surveyor, MDCCXLV.





The Explanation

A. *The Wall.*
B. *The* Vallum.
C. *The Ditch.*
D. *The Amphitheatre, greater diameter 50 yards, lesser 40.*
E. *The Church.*
F. *The Farmhouse.*
G. *The four City gates, which terminate the two chief streets.*
H. *The* Forum.
ik. *A Temple.*
l. *The Roman inscription.*
Mn. *A Fountain.*
O. *Onion hole.*
P. *Silver hill.*


G
H
P
G
G
A scale of 50 statute poles.
10
20
30
40
50
J. Mynde sc

over them four or five rows of flints; then usually a double row of stones, sometimes three rows, and at other times one only, laid in the same position; over these a like number of rows of flints, as before; and so alternately upwards. And a little to the westward of the south gate are yet to be seen seven of these ranges of stone, with six of flint between them; where the hight of the wall measured on the outside about eighteen feet. And about fifty yards eastward of the same gate are six ranges of stone, with five of flint between them; where a small part of the facing seems yet to be near intire. But there is no appearance either of copings, or battlements, on any part of the wall. Tho the ranges of stone in the front of the wall are placed horizontally, yet those within it often stand edgewise and somewhat obliquely, like the wall of *Severus* in the north of *England* (1). And at the south gate the thickness of the wall measured about five yards. From this account therefore it seems not improbable, that in the passage of *Leland*, given us by Mr. *Hearne* from *Stowe's transcript*, where it is said, *the wall without is in some place 6 or 7 fote highe* (2), for the numbers *6 or 7* should be read *16 or 17*.

The wall is not any where intirely demolished, except that two breaches have been made of late years on the north-west side, to open a passage for waggons.

(1) See Horsley's *Britann. Rom.* p. 123.
(2) *Ubi supra.*

waggons. And the ditch without the wall is in ſome places ten or twelve yards over, but in others at preſent not viſible; where probably it may have been filled up by the earth thrown into it from the *vallum*, that incompaſſed the city between that and the wall, and which is yet in ſeveral places of a conſiderable hight above the ditch. Each of theſe may be ſeen in the plan, (*a*) marked with the letters A, B, C. There is little appearance of the *vallum*, or military way, within the circuit of the wall, the ground being now more generally raiſed pretty near the top of the wall, on which grow many large oaks, and other timber trees. From the ſouth gate towards Wincheſter has lain a military road, which when broken up appears to have been pitched with flints.

The amphitheatre ſtands without the wall, at the north-eaſt corner, and diſtant from it upwards of an hundred yards; as appears in the plan, where it is marked by the letter D. Both the wall and ſeats, which are made in it, conſiſt of a mixture of clay and gravel. The wall is about twenty yards thick at the bottom below the ſeats, and decreaſes gradually to the thickneſs of about four yards at the top. There are five ranges of ſeats above one another, at the diſtance of about ſix feet on the ſlope. It has two paſſages into it, one towards the town, and the other oppoſite to it. The diameter of the area is fifty yards by forty, and the area itſelf now ſerves for a pond to a farmer's yard. The deſign of this amphitheatre might poſſibly be for the baiting

(*a*) *See* TAB. III.

ing of wild beafts, or other athletic diverfions; agreable to the cuftoms of thofe times. Tho at prefent no appearance of a cavern, or any other place proper for the reception of fuch animals, is to be difcover'd.

The area of the town within the walls contains at prefent only corn feilds, except a fmall quantity of meadow land, with an antient church, and farm houfe, near the eaft gate, both which are marked in the plan with the letters E and F. The method taken by Mr. *Stair*, in order to difcover where the ftreets formerly lay, was by obferving for feveral years before harveft thofe places, in which the corn was ftunted, and did not flourifh as in other parts. Thefe were very eafily diftinguifhed in a dry fummer, and run in ftrait lines croffing one another, as they are drawn in the plan. Moreover, by fpitting the ground, and often diging it up, he found a great deal of rubifh; with the plain ruins and foundations of houfes on each fide of thefe tracts. Whereas in the middle of the fquares nothing of that nature appeared, and the corn ufually flourifhed very well. The ploughmen alfo confirmed the fame, who found the earth harder, and more difficult to be turned up, in thefe tracts and near them, than elfewhere. And it is further obfervable, that two of thefe ftreets, which feemed rather wider than the reft, lead to the four gates of the city, one of them runing in a direct line from the north to the fouth gate, and the other from the eaft to the weft, which latter meafured at leaft eight yards acrofs. The four gates are marked with the letter G in the plan.

By diging likewife in different places Mr. *Stair* at length difcovered the ruins of a number of build-

ings

ings, in the form of a long ſquare, which in the plan is marked with the letter H. The foundations of ſome of theſe buildings were ſtill pretty intire, and the depth of them from wall to wall was found to be about twenty ſeven feet, and the breadth about ſixteen, which it is not improbable may be the remains of the antient *forum*. But between the letters *i* and *k* there appeared the foundation of ſome larger ſtructure, conſiſting of free ſtone three feet in thickneſs. And at *k* there ſeemed to be the pedeſtal or foundation of an altar, by the great quantity of aſhes and wood coals burnt, that lay round about it. What remained was about three feet in hight, four in length, and three in breadth. It conſiſted of large *Roman* bricks, one of which dug up intire, and communicated to me by Dr. *Collet*, is ſeventeen inches and a half long, twelve and a half broad, and two and a half thick; which accompanies this paper.

At the letter *l* was found the ſtone with the inſcription upon it mentioned above. And upon further ſearch Mr. *Stair* has ſince dug up within two feet of the ſame place, and about four feet under ground, a ſquare copper frame, compoſed of ſeveral mouldings, and its ſides ſoldered together, three of which are yet intire, but part of the fourth is broken. This frame incloſed a border of the ſame metal, one ſide of which is ſtill preſerved. The weight of them together is forty ſeven pounds; but the thickneſs of the frame varies in different parts from one fourth of an inch to much leſs, and the border is more than one eighth of an inch thick. Each ſide of the

frame

frame at the outer edge is about thirty three inches long. And from the ſize of the inner edge of the border, each ſide whereof is twenty inches and a half in length, it is ſuppoſed that the ſtone, which contains the inſcription, was at firſt placed behind it, and ſupported by it. And as that inſcription was erected in honour of *Hercules*, it might originally belong to the ſtone building, whoſe foundation is yet viſible, and which might have been a temple conſecrated to that deity by the perſons named in the inſcription; as was conjectured in the former account given of it. A draught of this frame and border in their preſent ſtate, taken by a ſcale of one ſixth of the original (1), now in the poſſeſſion of Dr. *Mead*, is prefixed to this diſcourſe (*a*). There was likewiſe a conſiderable number of braſs *Roman* coins found near the ſame place.

At the letter M in the plan was antiently a fountain, which at *n* diſcharged itſelf under the wall. Some of the ſtones, with which this fountain was incloſed, are ſtill to be ſeen.

And at the letter O is a large breach under the wall, two yards in length, and two feet in hight; tho

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(1) It may be neceſſary to repeat here, what was remarked in the *Errata* of the *Phil. Tranſ. N.* 474, which were put under the *Contents* of *N.* 475. That whereas *the draught of the Roman inſcription mentioned in p.* 201, *is there ſaid to have been taken by a ſcale of one fourth of the original; it was afterwards reduced by the ingraver in the plate to near one ſixth.* And in that proportion it is to be compared with the draught of the frame here given, wherein it is ſuppoſed to have been antiently placed.

(*a*) *See* TAB. IV. *Fig.* 4.

tho in the memory of ſome perſons yet living it was conſiderably higher. It is called *Onion hole*, from an imaginary giant of that name, who is ſaid once to have inhabited this city; and from whom likewiſe the *Roman* coins found there have been called *Onion penies*. The breadth of the wall thro this hole, where it is pretty much broken, meaſured now but ten feet; and being upon high ground could not, I think, have been a common ſewer, as ſome have imagined; but might originally perhaps have been deſigned for a private paſſage upon ſome neceſſary occaſions.

The moſt valuable coin, which has been diſcovered in the ruins of this antient *Roman* town, is a gold one of *Allectus* in fine preſervation, and very remarkable for a peculiar attribute of the deity on the reverſe. The front ſide repreſents the head of *Allectus* crowned with laurel, round which is this legend, IMP C ALLECTVS P F AVG. On the reverſe is placed the figure of *Apollo* with a radiated crown; his left hand, which holds a globe, has over it a whip; his right arm is raiſed in a forbiding poſture, and ſupports a *chlamys*, which croſſing his breaſt deſcends on both his ſides; at his feet ſit two captives, whoſe hands are tied behind them; and the legend round it is ORIENS AVG, with ML in the exergue (1). Theſe ſeveral attributes may be found in

(1) Some very ſkilful antiquaries have thought, that thoſe letters on the reverſe of many coins of the lower emperours, which are put

p. 666.

Fig. 1. p. 644.

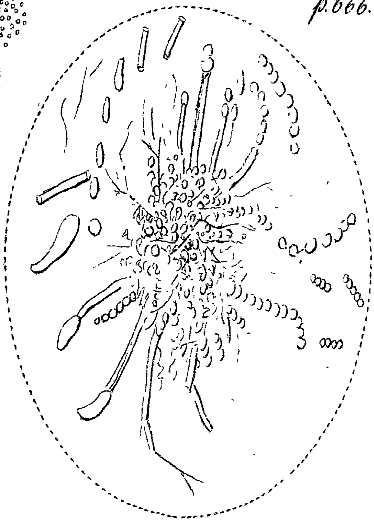

Fig. 2. p. 650.

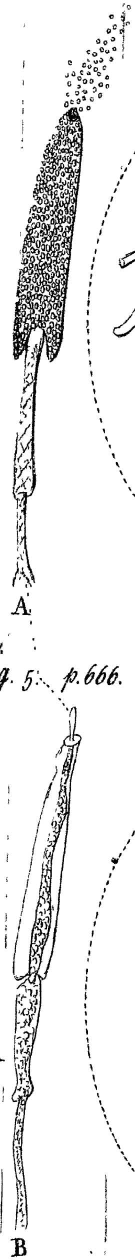

Fig. 5. p. 666.

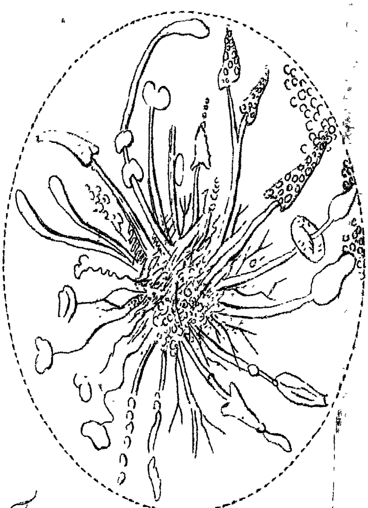

Fig. 3. p. 653.

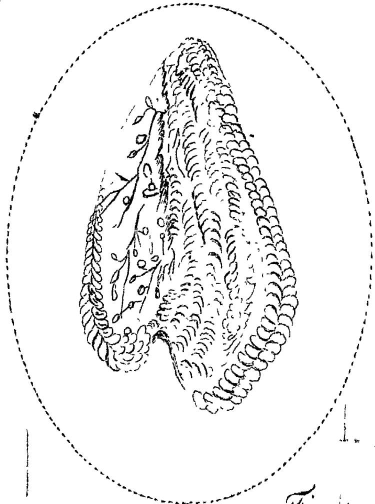

Fig. 4. p. 659.

in ſome or other of the imperial coins between the time of *Gordian* the younger and the *Conſtantines*, or later. But the ſingularity of the coin, I am now deſcribing, is this; that in the figure of *Apollo* the eyes ſeem plainly to be covered by a fillet, which goes croſs the forehead; the reaſon of which I cannot undertake to account for with certainty. But we are told by *Suetonius*, that among other reflections thrown upon *Auguſtus* for a ſecret entertainment made by him, at which the perſons preſent were dreſſed in the habit of deities, and this at a time of great ſcarcity in *Rome*, *Acclamatum eſt poſtridie frumentum omne deos commediſſe*; *et Caeſarem eſſe plane Apollinem, ſed tortorem.* To which the hiſtorian adds, *Quo cognomine is deus quadam in parte urbis colebatur* (1); which being the place where criminals were puniſhed, is thus deſcribed by *Martial*,

Cruenta pendent qua flagella tortorum (2).

It

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put at the bottom, often denote the place, where thoſe coins were ſtruck. And therefore, as I meet with ML by themſelves upon no others, but thoſe of *Carauſius* and *Allectus*, who both ruled in *Britain*; it ſeems not improbable, that they may ſtand for *moneta Londinenſis*, or *Londini*, ſupplying *ſignata* or *incuſa*. As upon ſome coins of *Conſtantinus Magnus*, who was firſt proclaimed emperour in *Britain*, we find MSL and MLI, which may alſo be ſo interpreted. Indeed the letters MLS and MLP occur upon the coins of ſome other emperours, where they have been read, and perhaps juſtly enough, *moneta Lugduni ſignata* and *percuſſa*.

(1) In *vit. Auguſt. cap.* 70.

(2) *Lib.* II. *epigr.* 17.

It was not unusual for the antients to apply the attributes of one deity to another on particular occasions. From whence one might be led to interpret this representation of *Apollo*, or the Sun, with a whip, and a bandage over his eyes (the emblems of *Justice*) together with the two captives, as descriptive of the punishment denounced against all, who should attempt to oppose the government of *Allectus*. And as the *Roman* coins do generally contain on their reverse some devise relating to the times, in which they were made; it seems not improbable from hence, as also from the legend, ORIENS AVGVSTI, that this was struck upon *Allectus's* first assuming the purple, after he had murthered *Carausius*: since upon others of his coins we meet with SPES AVGVSTI, VIRTVS AVGVSTI, PAX AVGVSTI, SALVS AVGVSTI, and the like, as denoting the gradual success of his affairs afterwards. An impression of this curious coin, which is now in the *museum* of Dr. *Mead*, is likewise given here (*a*). The place, where it was found, is marked in the plan by the letter P (*b*); which of late years has gained the name of the *Silver hill*, because more silver coins have been found there, than in any other part of the city. And by the remaining ruins, which discover themselves upon turning up the ground, it is supposed, that some large building stood antiently upon that spot. But great numbers of coins in all metals, and of all sizes, have likewise been found in several other places; so that

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(*a*) See TAB. IV. *Fig*. 5.   (*b*) See TAB. III.

that Mr. *Stair* is now poſſeſſed of ſeveral hundred, which have been all collected from this *Roman* ſettlement; among which are the emperours *Valentinian* and *Arcadius* in gold; with moſt of the imperial coins from *Auguſtus* to that time, either in ſilver or braſs; many of which are exceedingly well preſerved.

I SHALL only beg leave to ſubjoin here a breif account of an antient date in *Arabian* figures, which yet remains at *Walling* near *Aldermarſton* in *Berkſhire*. It is impreſſed in releivo upon a brick, near the top of a large and high chimney, on the outſide of a farmhouſe belonging to *William Wollaſcot* eſquire of *Woolhampton* in that neighbourhood. This date had always hitherto been read 1182, the two firſt figures, as they are ſeen from the ground, having both the appearance of a one; with this difference only, that the ſecond ſeems pretty much thicker than the firſt. And this led me upon viewing it in that ſituation to ſuſpect it might be a three, like that in the *Cambridge* date, publiſhed in the *Philoſophical Tranſactions*, *N*. 474. And accordingly having by the means of a long ladder an opportunity of going up to it, I found upon a near inſpection, that it was really ſo, as I had apprehended. For the ſmall curves in the ſecond figure being filled up with moſs gave it the appearance of a broad and ſtrait line, when ſeen at a conſiderable diſtance. I took an exact draught of the whole, by firſt impreſſing a paper upon the brick, with the ſeveral

ſeveral figures contained in it; and then delineating each of them carefully with a pencil. And the copy, which is here exhibited, is drawn by a ſcale of one third of the original (*a*). The houſe, where this date remains, is by tradition ſaid to have belonged antiently to a knight templer; but however that might be, the date muſt have been placed there long afterwards; as that order of knights was deſtroyed on the ſeventh of *January*, in the year MCCCVII.

G. C. Dec. 13, 1748.

John Ward.

(*a*) *See* TAB: IV. *Fig*. 6.

VI. *A Summary*

A Summary of ſome late Obſervations upon the Generation, Compoſition, *and* Decompoſition *of* Animal *and* Vegetable Subſtances; *Communicated in a Letter to* Martin Folkes *Eſq*; *Preſident of the* Royal Society, *by Mr*. Turbervill Needham, *Fellow of the ſame* Society.

Paris, *Nov.* 23, 1748, *N. S.*

S I R,

Read Dec. 15. 22. 1748.

§ 1. THO' I think myſelf now almoſt ſufficiently qualified, by the Multitude of Experiments I have already made upon animal and vegetable Subſtances, ſince the 16th, *N. S.* of laſt *March*, to lay down ſome certain Truths upon this Subject, and from them to advance, by Induction, farther than ſo ſhort a Period of Time would allow me to proceed by ſpecial Experiments, yet I would have your learned *Royal Society* look upon this Paper as an imperfect Sketch only of what I hope to publiſh from the Journals I have by me in a few Months, if theſe two or three Sheets are ſo fortunate as to meet with their Approbation. I am ſufficiently ſenſible how much I may hurt this little Performance, if I promiſe too much, and raiſe in this Matter higher Expectations from the Public than it may appear hereafter to deſerve: It is at this time therefore particularly the more neceſſary, that I ſhould be exceedingly cautious to advance no Propoſition raſhly; nothing, but what ſeems to flow naturally from Obſervation. But this Precaution, however

ſtrict, will not exclude now-and-then a probable Conſequence from appearing, provided it ſeems connected with ſome preceding manifeſt Truth; for ſuch muſt be allow'd, as proper Foundations for a more exact Inquiry in a Matter I am very far from pretending to have exhauſted. I muſt therefore obſerve, for my own Security againſt future Objections, that tho' I add no new deciſive Experiment to my preſent Liſt, or throw any more Light upon the Subject than what I have already amaſſed, I may poſſibly, before my Eſſay appears, whether by the Advice of Friends, or otherwiſe, conceive more mature Thoughts, reject ſome of the preſent, and adopt others in their Place. As this will be done, without affecting in any degree the main Syſtem, which I imagine turns upon unqueſtionable Truths, it is a Liberty I am perſuaded that equitable and learned *Society* will indulge me in, if no other Conſideration prevails, than the great Obſcurity that hangs over a Subject ſo extenſive and ſo intricate as this is; in which I am already engag'd much farther than I at firſt foreſaw, and indeed too far to recede without ſaying ſomething.

§ 2. I ſhall take as little Notice as may be, in this ſhort Summary, of the almoſt inevitable Miſtakes others may have made in this Matter before me, and the too haſty Conſequences they have drawn from Appearances that naturally ſurpriſe by their Novelty. Such Surprize is but too apt to captivate Perſons even of the moſt ſerene Thoughts, much more the young and unexperienced; ſuch as Mr. *Hartſoeker* was, when he firſt diſcover'd the ſpermatic Animals.

§ 3.

§ 3. Mr. *Lewenhoeck* indeed, so near his Cotemporary in this Discovery as to claim a Priority, was much more advanced in Age and Experience; yet if he should also appear to have been mistaken, we are not to be surprised at it; for his repeated Observations upon the Sperm of such a Variety of Animals, even as low as Insects, seem to intitle him to draw Consequences as extensive for a general System of Generation, as his Experiments had been. In effect, what two more powerful Arguments could a Philosopher with the Knowledge of no other Fact, than that of their Existence, have, than the Universality of Animalcules in this Fluid, and their seeming Confinement to this animal Secretion?

§ 4. The Method of Reasoning by Analogy is but too apt to lead us into Mistakes, and therefore we ought to be very diffident of Consequences deduced this Way. Every new Appearance that has no known Cause, immediately fixes, and, but toooften at last puts the Thoughts of the Observer upon the Rack. When the Mind arrives at this Intensity of Action, how natural is it to free ourselves from a painful Uncertainty at any rate, and that with as little Expence of Reflection as may be? The most obvious and easy Method is to class, if it admits it, and to reduce it to some other known *Phænomena*; possibly we are yet no nearer the physical Cause, because that of both is unknown. We have still, however, the Satisfaction to have diminished the Surprize it gives, by taking from its Singularity, and rest in some measure contented with this little Deceit.

§ 5. I call it a Deceit, if we acquiesce in it, till such time as a Number of Circumstances shall concur to

place

place it above the State of an Hypothesis, and shew us we have been right in our Inferences. Mere Analogy, founded only upon one or two Facts, and extended by Conjecture, however plausible, can but at most furnish Motives for a reasonable Doubt, and a more mature Enquiry. For tho', as a modern Author observes very well, Nature seems every-where to hold with itself, and go off by an almost imperceptible Gradation; yet, in our present Ignorance of the entire Chain of Beings, we are so liable to mistake two distant Species for the next immediate ones to each other, that the Analogy is thereby nearly extinguished, and its Traces almost effac'd.

§ 6. That this has been too much the Case in all the modern Systems of Generation, will appear I believe plain in the Course of this Memoir to every unbiass'd Naturalist. Animalcules were found universally in all animal Seed, almost at all times, and seemingly in this animal Secretion alone; they were therefore previously thought essential to Generation; or they should have added, a necessary Consequence of Properties in the Seed, which Properties were essential to Generation. But this Inference, however natural, was intirely overlook'd by them in their Reasoning; and Analogy induc'd them to stop at the first, without ever examining the second, tho' equally consequent. The Opinion of preexistent Germs had prevail'd, under the Notion of Female Eggs, ere this Discovery was made; and thus one Mistake had been grafted upon another. When the spermatic Animals appeared, it was not difficult to transfer these imaginary Germs from the one to the other; and at most Philosophers were

only divided by it; tho' as both Opinions were equally plausible, the latter generally prevail'd by its Novelty. The vast and unbounded Prospect it open'd to the Imagination, in a View of such a prodigious Series from the first Parent to the last, of original Lineaments, struck the Mind with an agreeable Surprize. The Folly of equivocal Generation, particularly as it had been stated by the Antients, the false Grounds they had proceeded upon to establish it, various Experiments that seem'd to prove every Animal, every Plant, descended from Individuals of the same Species; but, above all, the Facility of classing these spermatic Animals, the reducing them by Analogy to Seed and Eggs, and the known Transition of most Insects from one State to another, seem'd all sufficient to remove the Veil Nature had drawn, and furnish a Clue of a competent Length to conduct us into its most hidden Recesses.

§ 7. Thus this new System of Generation soon became a favourite Opinion of the last Age, as it is indeed still of this for the most part; and many ingenious Methods were imagined of answering the Difficulties from Observation that seem'd to oppose it. The more antient Hypothesis of female Eggs was at last blended with it, and both were work'd up into one System: Their real Existence was determined, with their Form, Colour, Size, Situation, and the Mechanism of their Conveyance to the Womb; and imaginary Valves were appointed in each Egg admitting one, exclusive of every other spermatic Animal. Happy the first of these minute Beings that could take Possession of this little Cell, and shut the Door against contending Millions! Hitherto

therto every Step seemed easy and natural, if not too closely examined; the Inquisitive were conducted as high as their Curiosity could promise; and we might have expected, that Philosophers should have stopp'd here; but there is no End of reasoning by Analogy.

§ 8. No Body of Men so strictly deserves the Name of a Republic as that of the Learned does: Every one is passionately fond of adding to the common Stock, and claims nothing in Return, but the Name and Merit of having enrich'd it; yet this Passion is often so violent, that base Metals are mistaken for Gold, and Pebbles for Diamonds. It is not therefore Matter of much Surprize, if some have carried the imaginary Scene yet farther; and, still proceeding by Analogy, have supposed that the reticular Expansion, observed in the Womb of Does some Days after Copulation, by *Harvey*, and since him, in other impregnated Females, was nothing more than the investing Web, spun by the spermatic Animal before it enter'd the chrysalidal State, and preparatory to its Transition from one Form to another. Certainly these Authors never consider'd the immense Disproportion, between the great Expansion of this Web and the inconceivable Minuteness of the Animalcule; otherwise it had appeared as rational to suppose, that an *Alpine* Mountain could have been rear'd in a few Days by a single Emmet successively pileing one Grain of Sand upon another. Nothing now seem'd wanting to complete this System, and place it above all Exception, but ocular Demonstration, if it might possibly be obtained, that the original Embryo was really contained

tained in each of these Animalcules: By Dissection, the young Butterfly had been observed in the Caterpillar three or four Days before it became a Chrysalid; Mr. *Lewenhoeck* had succeeded in some other very nice Operations upon extremely minute Subjects, nor did he despair of his Success in this; yet his repeated Attempts, it seems, all proved fruitless. But what the most exquisite Art had deny'd to *Lewenhoeck*, Chance, if we believe him, presented to another Naturalist, a little Man started from under the Integuments he was said to wear in his vermicular State; and the Observer very humourously gave us a Figure of this diminutive Entity perfect in every Member. These extraordinary Sallies, however, we must not place to the Account of the Learned, either of this or the last Age; they were generally exploded, and they indeed continue so; yet altho' they were peculiar only to the most lively; extravagant as they may appear to be, they were Consequences of the System; and thus was this Method of Reasoning by Analogy fairly pursued, as far as Imagination could carry it.

§ 9. *Cudworth*, *Grew*, *Le Clerc*, and some other Gentlemen of Judgment, had reflected too deeply upon Nature to give way to any Hypothesis, how plausible soever, that took in less than the whole Scene it exhibits to every attentive Observer. Yet they seem to have advanced much too far towards the other Extreme; and their System of plastic Natures, tho' in its Detail attended with many Proofs of extensive Thought, and profound Reflection, in a general View derogates as much from the

Omnipotence

Omnipotence of an All-wise Creator; and is not perhaps less extraordinary, than that Opinion which attributed the Regularity and Motion of the Planets to the Ministry of Angels. In this Light, I presume, it has been looked upon by others, as well as by myself; and it is upon this account that I imagine it has had so few Followers; I shall therefore take no further notice of it here, than to observe, that, inasmuch as it admits a productive Force in Nature, and Operations that go much deeper than a mere Developement of Parts, it has certainly more of Truth in it, than the Opinion of pre-existent Germs: as I flatter myself, will appear evident in the Course of this Memoir, by Arguments drawn not from Observations only, that are obvious to every Naturalist, but particular Experiments made upon animal and vegetable Substances, during the whole Summer of this present Year.

§ 10. To enter therefore more particularly into my Subject, where to place the pre-existent animal Embryo, for instance, whether in the Animalcule or Egg, was ever the Question, and still remains unanswer'd. A Division of vital, essential, and original *Stamina* or Lineaments was impossible; yet innumerable Instances in Monsters, Mules, and many natural Subjects, concur to prove, that the young *Fœtus* partakes of the Nature, Qualities, Constitution, Form, and Features of both the Parents; even as far as their Defects and Diseases, which are but too often hereditary. How can it then be agreeable to Reason? Or to what Purpose should we call in to our Aid unalterable original *Stamina?* Can the visible Species of any Production be determin'd

min'd by them, if every ſenſible Quality may be influenc'd indiſcriminately by either Parent? And if they cannot be alter'd, nor the viſible Species be determin'd exactly by them, in what does their Eſſence conſiſt, or how can they be applied to that very Uſe we ſeem to think them deſigned for? If they are placed in the Animalcule, or in the Egg, how are they tranſmitted? And if in the Animalcule, why is the Proceſs attended with ſo vaſt an Expence, ſo great a Waſte of Millions of Entities, each containing within itſelf a Series of the moſt perfect and moſt wonderful Productions in Nature, when one only of theſe Millions of Millions is alone to take Place? How are theſe Animals generated? if in the common Way, not only the Proceſs will be boundleſs, and theſe in their Seed have others, and ſo on in an immenſe Series; but they can not then be unalterable, becauſe they are ſuppoſed capable of being generated. Further, if they float in the Air, or lie hidden in Food, as ſome have thought, how is it that the *Stamina* of one Species do not ſometimes inſinuate themſelves into a ſtrange Parent, with all the Inconveniences and Abſurdities of equivocal Generation? Or if they are ſaid to be excluded by proper Strainers adapted for that very Purpoſe in diſtant Species; at leaſt they cannot be ſo in thoſe Kinds that are near a-kin: For if the ſpermatic Animal, which is naturally productive of a Horſe in its own proper *Matrix*, is yet ſo fitted to the Eggs of the Aſs, that it can poſſeſs a Cellule there excluſive of every other, which argues an exact Coaptitude, certainly the ſame Animalcules, if contained either in Food, Air, or Water, common to both Horſe and Aſs, might paſs the

Strainers indiſcriminately of either; and thus might we have Mules common from each reſpective Male, without a promiſcuous Congreſs of theſe two Species.

§ 11. In another View, if we conſider the extreme Tenuity, I may ſay the mere Nothingneſs of one of theſe *Stamina*, in its firſt Origin, at the Diſtance of many Ages; comparatively to any one Part, the ſmalleſt muſcular Fibre, for inſtance, of an adult Animal it is now ſaid to conſtitute: how can we underſtand, that ſo minute a Filament could be developed, or in any Senſe ſerve as a *Subſtratum* to a Cylinder ſo ſolid, ſo maſſive, ſo comparatively immenſe? Could a Mountain be look'd upon as a Superſtructure upon a Grain of Sand? Or the terraqueous Globe derive its preſent Dimenſions from the Dilatation of an Atom? What is not the prodigious Force of this muſcular Fibre in its preſent State, if compared with what it had in its Origin? and, conſequently, what muſt have been the Increaſe of real extraneous Matter, either by Appoſition, or Incorporation; which is now as much a Part of this Fibre as the original *Stamen*? And if thus much can mechanically be aſſimilated, why not the whole of it formed by mechanical Cauſes? Or why muſt ſo inſignificant a Part of it be ſaid to be concreated with the Univerſe? But to ſtrike at once with what, in my Opinion, may be look'd upon as a demonſtrative Argument againſt the Syſtem of original *Stamina*? The Difficulty ſtill increaſes immenſely, if we look into the Vegetation of Plants, and the wonderful Re-production of the Parts of Polypes, Starfiſh, Lobſters Claws, *&c.* The original

original *Stamina*, how minute ſoever, queſtionleſs are diffuſed through the whole Production; ſince in this Syſtem all animal or vegetable Growth is made by Developement only: But if diffus'd, then ſome or all maybe by ſucceſſive Biſection loſt; and if loſt, how can they be reproduc'd? Or if reproduc'd, why ever ſaid to be original, and concreated with the Univerſe?

§ 12. Theſe are but a few of thoſe many Difficulties that might be enumerated; which yet are of ſuch a Nature, that it is evident to every unbiaſs'd Obſerver, they cannot be even ſeemingly evaded, but by multiplying Suppoſitions on Suppoſitions; which at laſt render the Hypotheſis ſo complex, as to retain no one Characteriſtic impreſs'd upon the ordinary Proceſs and Operations of Nature. Is it not much more reaſonable to ſay, that ſo many ſecretory Ducts, ſo many Strainers, ſo many preparatory Veſſels in Animals, and ſuch a curious Diſpoſition in Plants for the Continuation of every Species, imply a Digeſtion, Secretion, and Preparation of Principles invariably, univocally productive of every Individual, when they fall into their reſpective *Matrices*, and find Aliment proper to aſſimilate? Are not theſe Principles contained in the Nouriſhment taken by the Parent Plant or Animal, the ſame that continually vegetate within it, and furniſh it with Materials for its own Increaſe; continue to be diſtributed till it becomes adult, then plentifully exuberate, whilſt it is, by new Preparations, fitted to propagate invariably in a proper *Matrix* its reſpective Kind? Elſe, why this Digeſtion? why this Secretion? why ſo many Strainers, Receivers, Ducts, and Valves? and why is ſome Food more productive of theſe Princi-

ples than others? Or if they are pre-existent Germs that are secreted, are the pre-existent Germs of every Species contained in every Bird, Beast, Fish, or Plant, that supplies another with nutritive Juice, and becomes its Food? What a strange Confusion? How unlike that beautiful Simplicity, which Nature exhibits in all its Productions? Germs shut up within Germs, and Nature swarming with supernumerary Entities, all which we readily conceive might have been struck out at once, when the Universe was created; yet pretend not to be able to understand how they may be continually formed in Times successive, and as Occasions may require.

§ 13. This should seem as unnatural, and as unphilosophical, as it is disagreeable to Observation: For if every mix'd Body is made up by the Combination of certain Principles, I think we cannot question; but that God may have established Forces in Nature, subsisting Forces, by which such Principles may, in certain Circumstances, be invariably united, without any Danger of deviating, so as to render Generation equivocal; and if every Production in Question is a mix'd Body, as it certainly is, we know at the same time, that, how various soever they are, a small Number of Principles differently combined will yield an inconceivable Variety, sufficient to produce them all. Thus may we reduce Nature to what it is really ever found to be, simple in the Beginning of its Course, but magnificent beyond Expression when distributed: And this, I believe, will readily be allowed to be its true Process in Generation, if, besides taking in all the ordinary *Phænomena*, which no Hypothesis could yet explain,

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this Proceſs is found conſonant to many particular Experiments, ſome of which ſeem to me to render the Syſtem inconteſtable.

§ 14. Modern Naturaliſts have unanimouſly agreed to lay down, for a certain Truth, that every Plant proceeds from its ſpecific Seed, every Animal from an Egg, or ſomething analogous, pre-exiſtent in a Parent of the ſame kind. If it is ever of Uſe to ſeparate diſagreeing Ideas, and previouſly to explain equivocal Words, it is particularly requiſite in this Caſe to determine what we mean by Seeds and Eggs. Seeds and Eggs, in the common Acceptation of thoſe Terms, are certain mix'd Bodies, of ſeveral Dimenſions, that immediately furniſh theſe Productions. In this Senſe they are underſtood to contain not only the pre-exiſtent Germ, but the *Nidus* alſo, if I may ſo term it, fitted for its Reception, and a due Supply of alimentary Principles to be aſſimilated in proper Circumſtances. They are therefore thus far heterogeneous Bodies, that coaleſce in a known Time; and their Principles are ſo far from being originally united at the Creation, that they ſenſibly come together from very diſtant Places in all hermaphrodite Plants, and from different Individuals in all thoſe Species, where the Male and Female are diſtinct. Now I cannot perſuade myſelf, but that either I have not underſtood what has been written on this Subject, or that Authors have not ſufficiently reflected upon this, when they aſſert, that, becauſe the *Plantula* is found in the Seed, an Oak, for inſtance, in an Acorn, that therefore this diminutive Tree bears likewiſe its Acorns, and thus on through a long Series.

Series. I ſhall not ask how this ſmall Plant can have Seed; in the common Acceptation of that Term, it is plain it cannot: and if it has not, where the pre-exiſtent Germ is lodged; how, from an Atom, at ſo immenſe a Remove, can it be increas'd to a ſenſible Maſs, and be ſucceſſively developed through ſo many Generations, till its Time of Appearance? with many other Conſequences that may be drawn from hence againſt the Reality of pre-exiſtent Germs; all which are too obvious to require a diſtinct Enumeration.

§ 15. It is in vain for us to pretend to lay down any one certain uniform Rule, and ſay to Nature, This is thy Scheme; ſuch are thy Statutes; and from theſe thou ſhalt not deviate. If in many Productions ſhe fixes it as an inviolable Law, that no Individual of that Species ſhall appear without a Co-operation of two Parents a Male and a Female, ſhe has at the ſame time her Hermaphrodites both in Plants and Animals; and if in theſe Hermaphrodites the two Sexes are yet ſo diſtinct, that ſhe ſeems but to have a little diverſified her Operations, without any ſenſible Deviation from her primitive Law, ſhe will, in another Inſtance, that of the *Pucerons* obſerv'd by Mr. *Bonnet*, act either with or without the Cooperation of a Male. If again you ſay that a Female may be impregnated, ſo that the Impregnation ſhall diffuſe itſelf, and penetrate as far as five or ſix Generations, ſhe will point out to you in the Claſs of Polypes many Kinds, where Generation is carried on without either Male or Female, Egg or Seed; tho', among theſe, there are ſome of the plumed Sort, where a whole Family, when by real Vegetation

getation branch'd out as far as Nature designs, jointly concurs to give one Egg, or something analogous to an Egg, as the Source of a future Progeny. And thus is this Class united to its next most immediate Superior. If you should still insist, that the vital essential *Stamina* of every Plant and Animal were really concreated with the Universe, and are now diffused in Water, Earth, or Air, from whence each will be united to its proper Subject in due time; or that the Experiments of *Niewentyt*, and other Naturalists, of the Stems and Roots of Beans, or other Seeds, altering their Directions several times whendisplac'd, to recover each its own, the Root downwards, and Stem upwards; that these I say evidently prove vital, essential, unchangeable *Stamina*; as they must be, if original, and concreated with the Universe: Instances might be brought from the Memoirs of the *Royal Academy* of *Sciences* at *Paris*, of Trees that have been so inverted, and induc'd to change their Direction, that the Branches have become Roots, and the Roots Branches; a *Phænomenon* totally inconsistent with vital, essential, and unalterable *Stamina*. In fine, if at last you resolve to stand by this one Resource, that at least every Individual proceeds from a Parent like itself; that the original Germs, tho' not wholly unchangeable, are yet sufficiently fix'd to determine every Species, and that they are either lodg'd in these Parents, or secreted from the Elements by Strainers through their Bodies: I believe I can furnish, from my last Summer's Observations, a Cloud of Instances, of a new Class of Beings, whose Origin has hitherto been unknown, wherein Animals grow upon, are produc'd by, and, in the

the ſtrict Senſe of the Word, brought forth from Plants; then by a ſtrange Viciſſitude again become Plants of another Kind, theſe again Animals of another, and thus on for a Series, further than the utmoſt Power of Glaſſes can carry the moſt inquiſitive Obſerver.

§ 16. It has generally been thought by Naturaliſts, that microſcopical Animalcules were generated from Eggs tranſported through the Air, or depoſited by a Parent Fly, inviſible to the naked Eye, or even that aſſiſted with Microſcopes. Yet is it ſtrange that no Naturaliſt ſhould yet have ſeen them, if they are really ſo numerous, when their ſuppoſed Progeny is ſo various, and themſelves muſt be thought to be ſo frequently gliding over the Surface of all ſtagnant Waters. By what extraordinary Turn is it brought about, might a Naturaliſt obſerve, that ſuch ſurpriſing Revolutions ſhould happen in theſe little Oceans, as a total Diſappearance of one Species followed by the almoſt immediate Succeſſion of another; and that in a manner ſo ſudden and unexpected, that I know not whither they are retired, or what new Forms they may have aſſumed. If they die, does a whole Race periſh together, without any known Cauſe? Or if they have taken any new Form, how is it that I ſee none of them altering, juſt alter'd, or expanding their little Wings upon theſe Waters, wherein I lately ſaw ſo many Millions in an aquatic State? If it is poſſible for them to become flying Inſects in a manner totally inviſible, why do not theſe new Parents again depoſit their Spawn in the ſame Waters, and give a Succeſſion of the late Species, that has diſappear'd?

peared? The Element is not unfit for a new Progeny, ſince other Kinds ſucceed in it; nay I can tranſport from neighbouring Infuſions ſome of the ſame ſpecific Animalcules into theſe abandon'd Infuſions, and they will live. Nor yet has the Generation of this Species any peculiar Seaſon which confines it: A freſh Infuſion of the ſame animal or vegetable Subſtance I apply'd before, will give me again in a little time the very Kind I am enquiring after, and that as often as I think proper to add new Matter. Thus might any Naturaliſt have reaſon'd, who had obſerved theſe Animalcules with ſome Attention; and been gradually conducted to doubt of their ſuppoſed Origin from flying Inſects, or Eggs tranſported by the Winds.

§ 17. But there is yet a ſeverer Difficulty, that ſprings from the Conſideration of Paſte-Eels: Theſe Animals, Mr. *James Sherwood* and I, by performing a kind of ceſarean Operation upon them, had the Pleaſure to obſerve were viviparous; and the *Royal Society*, about the latter End of 1745, or Beginning of 1746, did us the Honour to give Attention to the Diſcovery, when Mr. *Sherwood's* Paper * was read, and the Experiments exhibited at one of its Meetings. I need not repeat what was at that time or has been ſince obſerved, where the Multiplication from one Eel once roſe to 106. It is ſufficient to obſerve, that theſe Animalcules muſt thence conſequently be thought to have arrived at their ultimate State of Perfection; no longer liable to change, or to live in any other State; too weighty, even the leaſt of them, to be buoy'd up by or tranſported

* See *Phil. Tranſ.* N°. 478, p. 67.

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transported through the Air, and too much of the aquatic kind to subsist out of Water, or to travel over dry Land, as I have often experienced, and any Gentleman may, by permitting the Water to evaporate. The Question therefore is, how, in a Mass from the clearest Spring-water, and the purest Wheat-Flour, heated as intensely as the Composition will admit, these Animalcules may be generated? It is not but that I think myself sufficiently enabled, by my Experiments and Observations, to answer all these Questions, and perhaps many more of greater Importance; but I have the strong Prejudice of near two learned Centuries, and the Opinions of Men of much more extensive Knowledge and Parts than myself, to stem and get over, before I can establish my own Sentiments upon this Subject; and therefore am willing to hope I shall not appear to have chosen a tedious and unnecessary Circuit, in tracing out the several Steps I have taken, to place my Conduct in a more rational Light. I must further observe, that I am obliged, previously to any of these Thoughts or Discoveries, to my Friend Mr. *Hill*, who translated and commented upon *Theophrastus* with so much Applause, for two Observations, made while I was at *London*, upon a Seed-Infusion he gave me, and the *Semen* of a Dog in his own House, which I, and some other Friends of the *Society*, saw; a Peculiarity singular enough was, that the Animalcules seem'd all hamper'd, and in some measure adhering by their supposed Tails, struggling as it were with a kind of oscillatory Motion to disengage themselves, and not advancing at all progressively. The Consequence of this Observation, which sufficiently hinted that they were then

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enaſcent, and that their Tails were no Members given them by Nature to ſteer or ſwim withal, yet then eſcaped our Notice; and was not plainly clear'd up, till other ſimilar and more diſtinct Obſervations upon this Claſs of Animalcules occurr'd ſome conſiderable Time after.

§ 18. It is now Time to obſerve how much I am obliged to Mr. *de Buffon*'s Penetration, who firſt engaged me in this Enquiry, by his ingenious Syſtem, which he was pleas'd to read to me, and at the ſame time expreſſed his Deſire I ſhould purſue it, before I had myſelf any Thoughts of it, or any one Experiment had been try'd. He had been long diſſatisfy'd with the Opinion of pre-exiſtent Germs in Nature; and he and Mr. *Maupertuis*, Preſident of the *Academy of Sciences* at *Berlin*, had often diſcours'd together upon the Subject. We have ſeveral Hints of this Diſſatisfaction, in a little Book, publiſhed by Mr. *Maupertuis* himſelf upon this Queſtion at *Paris*, before my Arrival there; in ſhort, it was by general Reflections, and ſome other conſequent Thoughts, that Mr. *de Buffon* was conducted to frame his Syſtem of organical Parts. Theſe he ſuppoſed, by Coalition, to conſtitute the *prima Stamina* of all animal and vegetable Bodies, ſimple, uniform, common to all, and conſequently to be found in a certain Quantity in every Portion of Food, Aliment, or nutritive Juice; and from thence to be digeſted, and when the Subject became adult, ſecreted, and ſtrain'd, for the Formation of the Seed of every Plant and Animal; and in this Fluid or Subſtance to be conſequently found in much Abundance. He further ſuppoſed theſe organical Parts to be moving when diſengaged, living in Appearance, and gifted with certain Organs, but

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extremely ſimple in their Compoſition; being perhaps little more than elaſtic Springs more or leſs compreſs'd, more or leſs diverſify'd in the Direction of their Force. He thought the Calamary Machines I obſerved ſome time ago to be ſtrong Proofs of his Opinion; and the ſpermatic Animalcules to be Machines, or organical Parts like theſe.

§ 19. For my own part, I was then, as I had been before, ſo far of his Opinion, as to think there were compound Bodies in Nature, not riſing above the Condition of Machines, which yet might ſeem to be alive, and ſpontaneous in their Motions; ſuch as the calamary Machines would certainly appear, if they were render'd ſo diminutive as to conceal their Mechaniſm, and ſuch I then ſuſpected the ſpermatic Animals to be: for Motion in general was but an equivocal Argument, and did not neceſſarily imply Life in the common Acceptation of that Term When, for a further Proof, I inſtanced Mr. *Hill's* Seed-Infuſion, wherein many Bodies were ſeen to move in a manner very different from Atoms in a fermenting Liquid, and yet not ſo ſeemingly ſpontaneous as microſcopical Animalcules, he added, that in his Syſtem it muſt be ſo; that theſe were detached organical Parts, and that the Seeds, and particularly the Germs of Seeds in Plants, muſt neceſſarily abound with them more than any other Subſtances. Thus did our Enquiry commence upon Seed-Infuſions, from a Deſire Mr. *de Buffon* had to find out the organical Parts, and I, if poſſible, to diſcover which among theſe moving Bodies were ſtrictly to be look'd upon as Animals, and which to be accounted mere Machines. In the Courſe of this

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Paper I ſhall be as exact as poſſible, in philoſophical Juſtice. Whatever Experiments or Diſcoveries are to be aſcribed to Mr. *de Buffon*, were the Reſult of his Directions, or jointly made with him, I ſhall ſo ſpecify, that they may appear diſtinguiſh'd from all thoſe others I made at home. The four firſt Infuſions, among them one of Almond-Germs carefully pick'd out from between the two Lobes and Kernel, I mixed up at my own Lodgings, and then clos'd them in Phials with Corks. The Obſervations that occurr'd, were, firſt, a Separation or Digeſtion of the Parts of theſe Subſtances, and a continual flying off of the moſt volatile. Theſe offuſcated my Glaſſes at every Inſtant, and, according to the Mixtures, yielded a fetid or an agreeable Odour; particularly that of the Almond-Germs, one ſtrongly ſpirituous. Eight Days after they had been infus'd, I began to perceive a languid Motion in ſome of the Seed-Particles, that before ſeemed dead; ſuch as gave me Encouragement to proſecute my Enquiry. It was viſible, that the Motion, tho' it had then no one Characteriſtic of Spontaneity, yet ſprung from an Effort of ſomething teeming as it were within the Particle, and not from any Fermentation in the Liquid, or other extraneous Cauſe. A diſtinct Atom would often detach itſelf from others of the ſame or leſs Dimenſions; and whilſt theſe others remained abſolutely unmov'd, advance progreſſively for the Space of eight or ten of its own Diameters, or move in a little Orbit, then fall off languid, reſt between two others, and detach itſelf again and again, with a Continuation of the the ſame Phænomena. The Conſequences of theſe were obvious, the Motion was not ſpontaneous; for

for theſe Atoms avoided no Obſtacle, nor had any other Characteriſtic of Spontaneity. It was not from any Commotion in the Fluid, Fermentation or the flying off of volatile Parts; becauſe a large Atom would frequently move and detach itſelf from a much leſs abſolutely quieſcent: They did not ſeem to be enaſcent Embryo Animals, from a Depoſition of any extraneous Spawn; for the Phials had been cloſed with Corks; nay they were the very Seed, or the Almond-Germ Particles themſelves.

§ 20. Theſe ſame Obſervations Mr. *de Buffon* made himſelf; for we examin'd theſe Infuſions together a ſecond time at his own Houſe; and then it was that he order'd fifteen Seed-Infuſions to be made up, which we continued regularly to examine twice a Week, till I propoſed to him to take them home, and follow them more cloſely by a daily or hourly Inſpection, if neceſſary. The Reſult of our firſt Obſervations was, that tho' the Phials had been cloſe ſtopp'd, and all Communication with the exterior Air prevented, yet, in about fifteen Days Time, the Infuſions ſwarm'd with Clouds of moving Atoms, ſo ſmall, and ſo prodigiouſly active; that tho' we made uſe of a Magnifier of not much above half a Line focal Diſtance, yet I am perſuaded nothing but their vaſt Multitude render'd them viſible. It ſeem'd therefore as if the firſt teeming languid Particles we had obſerved, vaſt in their Dimenſions, if compared with thoſe we now ſaw, had broke and divided into this immenſe Multitude of microſcopical active Atoms. Then it was that we began to lay down a Diſtinction between animated and mere organiz'd Bodies; which, tho' far from being

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at this time groundleſs, yet afterwards proved to be falſe. Theſe, and the ſpermatic Animals, we ſuppoſed to be of the latter kind; and to be produc'd in their reſpective Fluids, by a Coalition of active Principles, much as I had ſeen the Calamary Machines form'd by Hundreds, tho' abſolutely detach'd, and ſwimming at Liberty in the Milt of the Fiſh: whilſt we thought on the contrary, that the ordinary microſcopical Animalcules, with ſtrong Characteriſtics of ſpontaneous Motion and Animation, were to be claſs'd among Animals, and imagin'd them to proceed from Parent Individuals of their own Species. It was not till ſome time after this, that, determin'd to convince myſelf and others, without any Poſſibility of Doubt, whether theſe moving Atoms were really produced from without, or from the very Subſtance infus'd: I diſcover'd all the common microſcopical Animalcules, the ſpermatic ones not excepted, were to be rang'd in the ſame Claſs, and that their Generation was very different from that of all other animated Beings.

§ 21. For my Purpoſe therefore, I took a Quantity of Mutton-Gravy hot from the Fire, and ſhut it up in a Phial, cloſ'd up with a Cork ſo well maſticated, that my Precautions amounted to as much as if I had ſealed my Phial hermetically. I thus effectually excluded the exterior Air, that it might not be ſaid my moving Bodies drew their Origin from Inſects, or Eggs floating in the Atmoſphere. I would not inſtil any Water, leſt, without giving it as intenſe a Degree of Heat, it might be thought theſe Productions were convey'd through that Element. Seeds or Plants were for this Rea-
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ſon improper, becauſe they might have been judg'd to have been previouſly adhering to theſe Plants or Seeds: I neglected no Precaution, even as far as to heat violently in hot Aſhes the Body of the Phial; that if any thing exiſted, even in that little Portion of Air which filled up the Neck, it might be deſtroy'd, and loſe its productive Faculty. Nothing therefore could anſwer my Purpoſe of excluding every Objection, better than hot roaſt-Meat Gravy ſecur'd in this manner, and expoſed for ſome Days to the Summer-Heat: and as I was determined not to open it, till I might reaſonably conclude, whether, by its own Principles, it was productive of any thing, I allow'd ſufficient Time for that Purpoſe to this pure unmix'd Quinteſſence, if I may ſo call it, of an animal Body. From this time I take Corruption intirely in a philoſophical Senſe, for the riſing of a dead Subſtance, by a new kind of Vegetation, into Life: and no Axiom, how much ſoever it may have been exploded, is more true than that of the Antients, *Corruptio unius eſt Generatio alterius*; though they drew it from falſe Principles, and ſo eſtabliſhed it as to render Generation equivocal, and never penetrated ſufficiently into Nature by Microſcopes, to diſcover this Claſs of Beings, that are neither generated nor generate in the common Way, yet furniſh a Key to lead to the Generation of all others. My Phial ſwarm'd with Life, and microſcopical Animals of moſt Dimenſions, from ſome of the largeſt I had ever ſeen, to ſome of the leaſt. The very firſt Drop I uſed, upon opening it, yielded me Multitudes perfectly form'd, animated, and ſpontaneous in all their Motions: And thus was I obliged to abandon not only the

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Notion preconceiv'd of a Distinction to be made in this Class of Animals, between those that appeared under a sensible Angle in the Microscope, and the atomical ones; but even that Hypothesis also which I had advanc'd as probable, in the little Essay I published in 1745, that spermatic Animals were no more than Multitudes of such Machines as those of the Calamary; for now it was plain of what kind they were, and whence they deriv'd their Origin.

§ 22. I shall not at this present time trouble you with a Detail of Observations upon three or four Scores of different Infusions of animal and vegetable Substances, posterior to these upon Mutton-Gravy; all which constantly gave me the same Phænomena with little Variation, and were uniform in their general Result: These may better appear at Length upon some other Occasion; let it suffice for the present to take notice, that the Phials, clos'd or not clos'd, the Water previously boil'd or not boil'd, the Infusions permitted to teem, and then plac'd upon hot Ashes to destroy their Productions, or proceeding in their Vegetation without Intermission, appear'd to be so nearly the same, that, after a little time, I neglected every Precaution of this kind, as plainly unnecessary. I take no notice yet of their Manner of being generated and generating; in relating these Discoveries, as I believe I shall be more intelligible, if I follow the Order of Time: It is a Justice moreover I owe both to Mr. *de Buffon* and myself; for some were made by him alone, some by me, and some of them in Concert together: His System, the Detail of his System, his Experiments, my own Discoveries, my Thoughts

in consequence of these Discoveries; all these were reciprocally communicated; we made a Secret of nothing to each other. Thus where one Truth seems to lead to, or is the natural Consequence of another, it will be easy, from the Order I have observ'd, to see how much I have been obliged to his Penetration and Foresight. But this will yet appear more distinctly, when our several Essays upon this Subject shall appear; and in the second Volume of his Natural History, which will very soon be published, I must declare for a Fact, that all which precedes his Accounts of the Experiments, begun *March* 16. *N. S.* of this present Year 1748, was previous either to his own Experiments or mine, and was read to me by himself.

§ 23. In this Order of Time therefore Mr. *de Buffon* not only repeated the Experiment I have taken notice of, and added particular Observations of his own, but made some intirely new in every respect, peculiar to himself. Among these, that never to be forgotten by Naturalists, which at once destroys the Opinion of Eggs in viviparous Animals, and shews the real Use of those reddish glandulous Bodies observed by *Vallisnieri* upon the Testicles or Ovaries, as hitherto call'd, of Cows. Every Anatomist knows, that the whitish Specks, near each of which a Hydatide is plac'd upon all Female Ovaries, were hitherto either look'd upon to contain the real Female Eggs, or to be the remaining Scars of Eggs fecundated and dislodg'd. *Vallisnieri*, nearer the Truth, thought the large reddish glandulous Bodies, which he calls Cherries, and found upon the Ovaries of Cows, and other Females, in the Time of their Heat, if the Animal is confined

to any particular Season, or at any Time, in those Females which are unconfined in this particular, were the real productive Organs contributory alone to Generation; yet still with a View to the antient Opinion of Eggs, for he supposed these glandulous Excrescences to be real oviparous Productions. Mr. *de Buffon*, on the contrary, long before Observation had realiz'd his Conjectures, rightly thought these to no more than temporary Blossoms, if I may so term them, not containing in their Cavity, which they have distinct when they are ripe, an Egg, but the real Female Seed; that the whitish Specks, scatter'd upon the Surface of Female Ovaries, were partly the remaining Scars of some of these temporary Blossoms now faded, as having perform'd their destin'd Office, or Embryo-Blossoms not yet expanded; that the Hydatid annexed to each of these contained a Quantity of imperfect indigested Seed; and that, if we took the Blossom in time, when it should be intirely ripe for Action, as when a Female is in Heat, or not barren, these red glandulous Excrescences would furnish a Fluid as really productive of true spermatic Animals, or organical Parts, as he calls them, as that of any Male observ'd by *Hartsoeker*, *Lewenhoeck*, or any other. The Result of these Conjectures was, that, ordering a Bitch in Heat to be strangled, and dissected immediately, we found two of these red Excrescences florid and ripe, one upon each Ovary, these, from their respective Cavities that ran obliquely under these Productions for near an Inch in Length, furnish'd a Tea-spoonful of a thick turbid Fluid; and this Fluid, observ'd in the Microscope with the most powerful Magnifier,

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after some little time exhibited Numbers of spermatic Animals, in every respect like to those hitherto observ'd by other Naturalists, animated, and moving spontaneously. Thus was Mr. *de Buffon*'s Conjecture verify'd in every Particular.

§ 24. About this Time, I think some few Days after, Mr. *de Buffon* in my Presence examin'd several Sorts of male *Semen*; and then it was that, for the first time, we fairly saw the spermatic Animals enascent. Those Kinds which satisfy'd us in this particular were extremely viscid, and contain'd in a certain Quantity in the Chrystal of a Watch. These Precautions are not unnecessary; for if a viscid Kind be not chosen, and that in a good Quantity together, such as that of Stags, *&c.* or any Seed of the least exalted Sort, if I may so term it, as we found some to be more so than others; it will alter in the Atmosphere by an Evaporation of its volatile Parts, which serve to hold it though but gently together, after which it will liquefy, vegetate, ramify into Filaments, and these Filaments again break into moving Globules, especially if the Weather be hot, before a small Portion can be adjusted to the Microscope: whereby an Observer may easily be imposed upon, and think the spermatic Animals original and pre-existent, because he could not discern that Action which produc'd them. This Deception takes Place in all *Semen* of the more exalted Kinds, such as particularly the Milt of Fish, when it is in a State of immediate Impregnation, and many others: For it is to be observed, that the *Semen* of Animals is not at all times in an equal State of Exaltation; and consequently that some Sorts,

or

or even the ſame at different times, will at ſome give the ſpermatic Animals immediately, but at others not ſo ſoon, and perhaps not under ſome Hours: which is the Reaſon why they have often been ſaid by Naturaliſts, and even by *Lewenhoeck* himſelf, not to have been found upon Inſpection. By this it will appear, that we had tried many Sorts, before we had the good Fortune to meet with one, in that exact Degree of Exaltation neceſſary to exhibit the whole Proceſs of this Vegetation; and ſo may others who ſhall be deſirous of trying theſe Experiments after us: Yet, when they ſhall at laſt have obtain'd a proper Subject, one accurate View will be ſufficient, and found to give the Key to the whole Secret.

§ 25. When we had ſeized this favourable Opportunity, we ſaw a ſmall Portion of male *Semen* plac'd on the Microſcroſcope, firſt, as it were to develope and liquefy, then ſhoot out into long Filaments, ramify on every Side, theſe open and divide into moving Globules, and trailing after them ſomething like long Tails; theſe Tails were ſo far from being Members given them to ſwim and ſteer by, that they evidently caus'd in them an inſtable oſcillatory Motion; and were in Effect nothing more than long Filaments of the viſcid ſeminal Subſtance which they neceſſarily trail'd after them; they were of various Lengths in various Animals, and they inſenſibly, by the continual progreſſive Motion of thoſe Animals, grew ſhorter and ſhorter, till ſome of them appear'd without any at all, ſwimming equably in the Fluid. It was then plain how theſe Animals were to be claſs'd; their Origin was clearly to be deriv'd from Principles contain'd in this

this Matter, either by an Evolution of organical Parts, as Mr. *de Buffon* ſuppoſed, or by a real Vegetation, as I thought, of the ſame kind with thoſe I had before obſerv'd in my Infuſions; tho' more prompt, becauſe the Matter was more exalted: conſequently the ſpermatic Animals were of the ſame kind as all other microſcopical Animals, their Origin the ſame, their Influence nothing more in Generation, nor any otherwiſe conducting to its Cauſe, than as Effects of thoſe Principles in the *Semen*, which alone are the true and adequate Cauſe of it. See *Fig.* 1.

Theſe vegetative Powers, which, from the very Beginning of my Obſervations, I had found to reſide in all Subſtances animal or vegetable, and in every Part of thoſe Subſtances, as far as the ſmalleſt microſcopical Point, I had at this time certain Proofs of; tho' not ſo plain and inconteſtable as thoſe I procur'd a few Days before Mr. *de Buffon* left *Paris* for the Country, and which I proſecuted after his Departure. Theſe I communicated to him in few Words the Night before he began his Journey, yet he was not at that time acquainted with any ſpecial Detail of the many Singularities that attend theſe latter Vegetations, for I had but juſt then made and enter'd upon the Diſcovery of them myſelf. I am obliged the more particularly to obſerve this, becauſe the many Conſequences he has ſince drawn, as well as myſelf, and which, without any mutual Communication, happen'd to tally with and ſeemingly to flow from the Diſcoveries, were not in Fact deduced from a circumſtantiated Knowlege of theſe new Phænomena, which he had not, but from this one Principle, *that there*

is

is a real productive Force in Nature; in which we had both long ſince agreed, however we may have differed in explaining that Action: For whether it be by an Evolution and Combination of organical Parts, as Mr. *de Buffon* ſuppoſes, or by a real vegetating Force reſiding in every microſcopical Point, may be probably far beyond the Power of Microſcopes to determine. But as the Principle from which we depart is intirely the ſame, it muſt neceſſarily lead to ſimilar Thoughts, and ſimilar Conſequences.

§ 26. My firſt Proofs therefore were drawn from a cloſe Attendance to all the common Infuſions, particularly that of Wheat pounded in a marble Mortar. It was plain from them all, that after ſome time allow'd to the Water to call off the Salts and volatile Parts, which evaporated copiouſly, the Subſtance became ſofter, more divided, and more attenuated: To the naked Eye, or to the Touch, it appear'd a gelatinous Matter, but in the Microſcope was ſeen to conſiſt of innumerable Filaments; and then it was that the Subſtance was in its higheſt Point of Exaltation, juſt breaking, as I may ſay, into Life. Theſe Filaments would ſwell from an interior Force ſo active, and ſo productive, that even before they reſolved into, or ſhed any moving Globules, they were perfect Zoophytes teeming with Life, and Self-moving.

If any Particle was originally very ſmall and ſpherical, as many among thoſe of the pounded Seeds were, it was highly agreeable to obſerve its little Star-like Form with Rays diverging on all Sides, and every Ray moving with extreme Vivacity. The

Extremities likewiſe of this gelatinous Subſtance exhibited the ſame Appearances, active beyond Expreſſion, bringing forth, and parting continually with, moving progreſſive Particles of various Forms, ſpherical, oval, oblong, and cylindrical, which advanced in all Directions ſpontaneouſly, and were the true microſcopical Animals ſo often obſerved by Naturaliſts. This brings to my Mind a Phænomenon often taken notice of, and ſeen with Surprize, Particles detach'd by the Reaction of the Water from the Extremities of the Fins of Muſſels, which yet continue to move progreſſively. I think it ſufficiently explain'd by theſe Obſervations; and that it is more than probable, that Muſſels, Polypes, and other Kinds of this Nature, vegetate in a Manner analogous to this gelatinous Matter. See *Fig*. 2.

§ 27. In the Infuſion of pounded Wheat, the firſt Appearances, after an Exhalation of volatile Parts, as in every other Infuſion, were the ſecond or third Day Clouds of moving Atoms, which I ſuppoſe to have been produced by a prompt Vegetation of the ſmalleſt and almoſt inſenſible Parts, and which requir'd not ſo long a Time to digeſt as the more groſs. Theſe in a Day or two more intirely diſappeared; all was then quiet, and nothing to be ſeen, but dead irregularly formed Particles, abſolutely unactive till about fourteen or fifteen Days after. From theſe uniting into one *Maſs* ſprung Filaments, Zoophytes all, and ſwelling from a Force lodged within each Fibre. Theſe were in various States, juſt as this Force had happen'd to diverſify them; ſome reſembled Pearl-Necklaces, and were a kind of microſcopical Coralloids; others were uniform throughout

throughout their whole Length, except juſt the very Extremity, which ſwell'd into a Head like a Reed, if the Force had acted equally on all Sides, or like the Head of a Bone at its Joint, if the Matter in its Expanſion had bore to either Side. Theſe Filaments were all Zoophytes, ſo teeming with Life, that whenever, upon taking a Drop from the Surface of this Infuſion, I had ſeparated the Extremity of a Filament ſo ſhort as not to conſiſt of above four or five Globules Chaplet-wiſe; they would advance progreſſively and in Concert, with a ſort of vermicular Motion, for a little Way, then fall off irregularly to one Side, as if not yet fitted for progreſſive Motion, languidly turn their Extremities, and then again lie quiet for ſome little time. It was my Fortune however, not in this Infuſion only, but in many others, to find ſome of theſe Chaplet-like Animals much ſmaller indeed than thoſe of the Wheat-Infuſion; but intirely regular, conſtant in their vermicular Motion, and which were conſequently arrived to a higher Degree of Maturity and Perfection. I own I cannot but wonder to this Day at what I ſaw; and tho' I have now ſeen them ſo often, I ſtill look upon them with new Surprize. Yet have theſe Phænomena ſerv'd me to very good purpoſe, and clear'd up many Difficulties in my former Obſervations.

The Origin of Blight in Wheat, Rye, and other Vegetables, was no longer myſterious: An Atmoſphere charg'd to an extraordinary Degree with Humidity, now plainly appear'd ſufficient, particularly while the Grains were tender and replete with a milky

Juice in a certain Degree of Exaltation, to produce in them this new kind of Vegetation, and to form their interior Subſtance into Filaments, which are indeed thoſe very Eels I obſerv'd ſome Years ago in blighted Wheat.

This agrees perfectly with another Obſervation made by the Gentleman who tranſlated my little Eſſay into *French*: Some of this blighted Wheat, two Years after I had gather'd it, I had given to Mr. *Trembley*, and he to this Gentleman. In a Note he has added, he obſerves, that theſe Filaments not only recover'd Life and Motion, after they had been ſo long dry, by macerating them in Water; but many broke, and diſcharg'd from within them Globules, which mov'd with extreme Vivacity. The Application of the foregoing Obſervations to this Caſe is eaſy and natural; nor is it now any Wonder, that theſe Filaments, the vegetative Force ſtill reſiding within them, ſhould move and reſolve into Globules, or that they ſhould have ſubſiſted ſo long, full of that kind of Life they are actuated with, though dry and without Nouriſhment; for now they ceaſe to be Eels, as I formerly thought them.

Blighted Rye, which is alſo ſo full of Filaments of this Nature, that the Grains are ſwell'd in their Diameters, and extended to an extraordinary Length by this new kind of Vegetation, exhibited nearly the ſame Phænomena when macerated, and is to be claſs'd accordingly. I am told by ſome of the Gentlemen of the *Royal Academy* of *Sciences* here, that in thoſe Provinces of *France*, where this blighted Rye abounds, and is made up into Bread; it produces very ſtrange Effects in the poor Country People who feed

feed upon it, many of which are here found in the Hoſpitals afflicted with a very ſingular kind of Mortification, which cauſes their Limbs to drop off.

There are two Sorts of Blight, in one of which the Grain crumbles into a black Powder; and the other is that which gives theſe moving Filaments or Eels. Mr. *Bernard de Juſſieu* tells me, that one is from a Corruption of the Flour, and the other of the Grain.

It may not here be amiſs to hazard a few Queries. Do not all Mortifications, and other Maladies in which there appears an extraordinary Exuberance of Matter in any one Part, proceed from a Weakneſs, a Want of Reſiſtance, and from Principles of Union, which give to this vegetative Force, found to reſide in every Point of animal or vegetable Subſtances, more Play in one Part than in another? For If the Reſiſtance be not equal in all Parts, the exuberant Matter muſt break forth, and cauſe that Part to decompoſe; and if the Habit of Body be extremely lax, the Decompoſition muſt continue; and that, in a certain extraordinary Degree, we ſhall call a Mortification. To rub a Wound, or any natural Sore, with Salt and Spirits, is found to be ſalutary, and preventive of Mortifications; and Salt I know, by Obſervation, will immediately put a Stop to theſe microſcopical Vegetations, and cauſe the Animals to ſubſide motionleſs to the Bottom: Therefore it is probable, that Salts and Spirits are Principles of Union, and productive of a greater Reſiſtance in the ductile Matter acted upon by this vegetative Force. High Living, rich Wines, *&c.* are Preſervatives againſt many contagious epidemical Diſtempers; Do not therefore theſe Maladies ariſe from a laxer

Habit of Body, and a more than ordinary Action of this ſame vegetative Force? And may not theſe, and many other Phænomena of this kind, be reduc'd to the ſame Principles? But this I leave to the Conſideration of Phyſicians, who are better Judges of the Extent of theſe Obſervations and Principles.

The Subſtance emitted from the Globules of the *Farina fæcundans* of all Flowers, by an Action I obſerved ſome Years ago, is alſo a Subſtance of this Nature, filamentous, and in a vegetating State: Nothing can reſemble it more than the Fibres of moſt kinds of Mould; reſolving all, as they do in Water, into others of a much finer Contexture, when the Vegetation, that had been before ſtopped by the nitrous Salts of the Atmoſphere, begins by the Aſſiſtance of the Water to act again: And I know, by Obſervation, that all kind of Mould is formed by a Proceſs of the ſame Nature as the Growth of theſe microſcopical Plants; and to be claſs'd conſequently with them, and reduc'd to the ſame Principles.

I cannot finiſh this Article without obſerving, that nothing can more perfectly than theſe wheaten Filaments, repreſent in Miniature Corals, Coralloids, and other Sea Plants, which have long been obſerv'd to be teeming alſo with Life, and have been ſuppos'd to be the Work of Animals, as it will appear to any one, that but inſpects the Figure I have annex'd, and recollects my Deſcription. Are not therefore all theſe in the ſame Claſs, and is not their Origin ſimilar? See *Fig.* 2.

§ 28. But theſe Inſtances from common Infuſions, of a vegetative Force reſiding in every microſcopical Point of animal or vegetable Matter, how

ſtrong

ſtrong ſoever and ſurprizing, were neither ſo wonderful or extraordinary as ſome others I obſerv'd after Mr. *de Buffon's* Departure. From the wheaten filamentous Zoophytes it was eaſy to infer, that they ſprung from, and were Productions of, the Maſs of Matter that had ſubſided to the Bottom of the Phial. Yet this I could not obtain a Sight of; nor was it poſſible in this Way to obſerve them without ſeparating them from their Roots and from the Maſs, out of which they aroſe. The Method the moſt natural therefore which occurr'd to me for the viewing of theſe Zoophytes, without diſturbing their Vegetation, and for obſerving their whole Proceſs, from the Origin of the Plants to their laſt Degree of Maturity, was to take extreme thin Slices of Cork, and inſert, through little Holes which I made, four or five in each Slice, Grains of Wheat or Barley, or any other farinaceous Seed, for theſe all nearly agree in the Phænomena they exhibit, with the Germ either turned upwards, or carefully pick'd out with the Point of a Penknife, to prevent their uſual ſhooting.

Theſe were permitted to ſwim upon the Surface of freſh Spring-water, in a Glaſs expoſed to the Sun, that the whole vegetating Force might be determin'd downwards towards the inferior Moiety of each Grain, which alone could in theſe Circumſtances imbibe and be ſaturated with Moiſture. This anſwer'd my Purpoſe intirely; my Plants grews downwards into the Water like Corals, but appear'd not till ſeveral Days after the Grains had been thus expos'd; and were at laſt ſo large and ſtrong, that I could ſee them with my naked Eye.

When

When they became thus visible, I cut off with a small Pair of Sciffars the vegetating Extremity, and plac'd it in a concave Object-Glass with Water. The Plants then took a new Direction, follow'd the Expanse of the Fluid, and continued to vegetate, while I supplied them with Water, which I did from time to time, covering them after Observation with another concave Object-Glass, to prevent the Fluid from evaporating too fast. Thus I had for the Subject of my Observations what I may call a microscopical Island, whose Plants and Animals soon become so familiar to me, that I knew every animal Species, and every individual Plant almost without any Danger of Mistake; an Exactness so necessary, that it would not otherwise have been possible to follow the Process of this Vegetation without Confusion. From this time I laid aside the Use of large Infusions, and provided a certain Number of Watch-Chrystals, or concave Object-Glasses, for every Portion of animal or vegetable Substance I was to macerate in Water. The Use of these is plain and easy; many fruitful little Islands of various Kinds with Labels and Dates affix'd to each may thus be obtain'd, by placing the vegetating Substances in these Glasses and this is the Method I would recommend to all those who shall be desirous to repeat or pursue my Experiments.

I find my Subject grows upon my Hands, and I am unwilling to take up too much of your Attention: I shall therefore finish these Observations by annexing a Figure of my Wheat-Island and its Productions, all which will be sufficiently intelligible without any more Words; and I shall reserve a

Multitude

Multitude of other Obſervations I have by me in my Journals, upon Infuſions and other vegetating Iſlands for the Eſſay, which I hope to publiſh in ſome Months, if theſe few Thoughts and Diſcoveries ſhall meet with Approbation. See *Fig*. 3.

§ 29. Yet muſt I treſpaſs for a few Pages more; I cannot conclude this Letter without laying down ſome general Truths, and recalling theſe ſcatter'd Remarks to ſome certain Principles. A few Propoſitions of this kind, together with the probable Conſequences, that ſeem naturally to flow from them, will not only make my Syſtem of Generation clear, but alſo take off many Objections, and render theſe very Obſervations better underſtood, when they are reduced under certain Heads.

It ſeems plain therefore, that there is a vegetative Force in every microſcopical Point of Matter, and every viſible Filament of which the whole animal or vegetable Texture conſiſts: And probably this Force extends much farther; for not only in all my Obſervations, the whole Subſtance, after a certain Separation of Salts and volatile Parts, divided into Filaments, and vegetated into numberleſs Zoophytes, which yielded all the ſeveral Species of common microſcopical Animals; but theſe very Animals alſo, after a certain time, ſubſided to the Bottom, became motionleſs, reſolv'd again into a gelatinous filamentous Subſtance, and gave Zoophytes and Animals of a leſſer Species.

This is not only true of all the common microſcopical Animalcules, but of the ſpermatic alſo; which, after loſing their Motion, and ſinking to the Bottom, again reſolved into Filaments, and again gave leſſer

Animals.

Animals. Thus the Proceſs went on through all viſible Degrees, till I could not any longer purſue them with my Glaſſes: And thus evidently the ſpermatic are to be claſs'd with the common microſcopical Animals.

Hence it is probable, that every animal or vegetable Subſtance advances as faſt as it can in its Reſolution to return by a ſlow Deſcent to one common Principle, the Source of all, a kind of univerſal *Semen*; whence its Atoms may return again, and aſcend to a new Life. This common Element therefore, tho' uniform in its Origin and homogeneous, branches out into innumerable Species more and more compounded, more and more heterogeneous, as they depart and are further from this Source of organiz'd Bodies; yet may a Particle often be arreſted, or moulded into other Bodies, long before it attains, which ſome perhaps never do, to this ultimate Reſolution. Nor is there any Danger upon theſe Suppoſitions of falling into equivocal Generation; becauſe the ſpecific *Semen* of one Animal can never be moulded into another, and Seeds may differ ſpecifically from one another by many inviſible Principles totally unknown to us, and unattainable by Experiments; for we are very certain that the Power of Glaſſes, or Force of any *Menſtruum* we can employ, muſt ſtill leave us at an immenſe Diſtance from the ultimate Reſolution of Bodies, in which alone they agree, and are homogeneous.

I ſay therefore the ſpecific Seed of one Animal can never give another of a different Species; for, to be this *ſpecific Seed*, it muſt have gone through many Changes from its firſt Origin, and have many Singularities

peculiar

peculiar to itself, and acquired since it passed from the homogeneous Element, in which all Kinds coincide. The active vegetative Force that resides in it must be precise, its Quantity must be exactly proportion'd to the Nature, Solidity, Tenacity, Quantity, and Resistance of the ductile Matter it has to wade through, if I may so express myself; and these Combinations are very different in different Subjects. Thus much the many Strainers in every animal Body, necessary to extract this *Semen* from the Aliment we daily digest, and to prepare it, seem evidently to imply. Yet is not this, sufficient as it may appear to cause Varieties in the several Species of *Semen*, all that is to be consider'd: Times and Circumstances make Changes in it even during the Term of Gestation. What does not the *Fœtus* then undergo? and who can determine the Differences between *Matrix* and *Matrix*; between the Matter that is assimilated into a *Fœtus* in one Subject, and that in another; between the fixing Principles, the Quantity of Salts, Spirits, *&c.* in a Parent of one Species, and one of another; between the more copious or more limited Affluences of assimilating Matter; and between Times, where even single Minutes, Instants, *&c.* may be of the greatest Consequence? I see the Whole indeed, but confusedly; yet do I see the Source of a Variety; which, boundless as it were, if permitted to expatiate at full Liberty, is nevertheless invariably confin'd, by Him who made and rules the Universe, to a certain determinate Number of Species. Time, Action, Season, Quantity of Force, Quantity of Resistance, fixing Principles, Affluence of assimilated Matter, Direction,

and numberlefs other Variations, are all employ'd for His Purpofes, and modell'd by that Almighty Power, which forms and directs the Whole.

Thus do thefe Principles, however capable of differing Combinations, yet admit only of a limited Variation, and never deviate further than is confiftent with univocal Generation. Monfters, Mules, imperfect *Fœtus's*, and other Inftances of this kind, are but rare; and as they can be afcrib'd to nothing fo properly as to the Obftacles they meet with, or to fome accidental infuperable Refiftance in the Matter of which they are formed, they do at leaft ferve to fhew that there is in Nature a real productive Force given it at its Creation; and that animal or vegetable Productions are not the Confequences of pre-exiftent Germs, plaftic Natures, or of the immediate Hand of God himfelf, any more than the moft regular Operations of the Planetary World.

§ 30. But to proceed in my Confequences from thefe Obfervations, all Naturalifts muft acknowlege, that the more compounded the organiz'd Bodies are, the lefs Danger there is of equivocal Generation in the Production of them; for thus the immediate Principles from which they fpring, and their Circumftances during the Time of Geftation, muft be much more varied than the more fimple Bodies are, and at the fame time be further removed, from that univerfal Element into which they may all ultimately be refolved: And even in the loweft Clafs of microfcopical Animals, I can truly fay, that I never yet obferved any others than Productions fpecifically determined; the fame Subftances giving the fame Plants and Animals, and in the fame uniform Order

der

der and Defcent. Neverthelefs, tho' thus fpecifically determin'd, no one, that obferves their Origin with the fame Care as I have done, will be inclined to afcribe it to pre-exiftent Germs: It is therefore probable, as I juft now advanced, that when we arrive at the loweft we can difcover in this Clafs, we are yet at an immenfe Remove from the univerfal Source; notwithftanding that fome of them are fmall beyond Conception, and no lefs fimple in their Motions; which argues their Organization as fimple, and feems to imply that there are among them, or not at a very great Diftance from them, fuch as are only mere Machines, without any true *Spontaneity*.

I have myfelf feen a vaft Gradation, and fuch a one as I have yet but an imperfect Notion of, in a Courfe of continual Obfervations made upon Infufions and Macerations of all kinds, from the moft compounded to the moft fimple; from Animals of the largeft kind to moving Atoms of the leaft; from Motions as flow to the moft powerful Magnifier, as the Motion of the Minute-Hand of a Watch to Eyes unarm'd; from free Progreffion in all Directions to merely ofcillatory Balances; which all feem to come to at laft in the Courfe of their Decompofition, when they are juft upon the Point of difappearing.

§ 31. Thus thefe Animalcules, if they may be call'd indifferently by that Name, manifeftly conftitute a Clafs apart; and their greateft Characteriftic is, that they neither are generated, fubfift by Nutriment, as other Plants and Animals do, or generate in the ordinary Way. This is indeed true, if the whole Clafs is taken in one general View: Yet is the Head of it united to the Species of the next im-

mediate Superior. The Bell-Animal, of which I have had many from my infused Substances, and whose Growth I have pursued from its first Origin, is a Species of microscopical Polype, generating and feeding as other Polypes do, when once itself is generated; tho' its own original Generation is perhaps different from that of the others; for I could never obtain any of the larger Kinds this Way. I say this however with some Reserve; for I will not assert but that some decay'd Water-Plants decomposing in particular Circumstances, and their Substance exuberateing, may perhaps, when urged by this vegetative Force, give Polypes of every Kind; nay I very much suspect, that several of the lowest Kinds of visible Animals may, in due Circumstances, which yet perhaps are rare, be recoverable this Way, when the whole Species has perished in particular Places by some uncommon Accident. This I the more readily believe, from the Reasonableness of some Allowances to be made in this respect; all which may be permitted; and must have been foreseen by the GREAT CREATOR, without any Danger of Confusion, or an unlimited Generation of new Species never before produced: He who made Nature, and sees through the whole Machine, well knew its utmost Force, and has consequently foreseen every Circumstance, and limited its Productions accordingly.

Nor indeed can there be a stronger Argument deriv'd from any System of Generation whatsoever, of an All-wise Being, All-powerful, and All-good, who gave to Nature its original Force, and now presides over it, than from the Consideration of an exuberating ductile Matter, actuated with a vegetative

tive Force, limited, tho' we know not its exact Bounds, in its specific Ascent or Descent, and expanding itself in Directions as certain and determinate, as the Motions of the Plants.

§ 32. These Thoughts will appear to be less hazarded, if due Attention is given to the Generation of the Paste-Eel. The *Royal Society* knows it to be viviparous; consequently perfect in this State, and such as may continue to generate in the common Way, as long as it has an Element and Matter proper for its Subsistence; yet is its own original Generation, as far as I can learn by Observation, as that of all these microscopical Animalcules, from a ductile vegetating Matter, the Produce of Wheat-Flour and Water; tho' it undergoes more Changes than others, and lives in other Conditions; ascending for some time before it enters its chryfalidal or Egg like State, whence it comes forth a perfect Eel. I have added a Figure of a Group of these Eel-Chrysalids, but the Detail of their Metamorphosis I shall reserve for my little Essay, and not trouble you now with an Account too circumstantiated of every Observation I have made upon them: Besides that I am not yet throughly satisfied in the whole Manner and Process of their Generation. See TAB. V. *Fig.* 4.

§ 33. But now, to obviate every Objection that may remain against the Existence of this vegetative Force, which seems to be the Key to much Knowledge, and to remove many Errors; it may be proper to add, that, besides ocular Demonstration, which any Naturalist may have, besides the Precautions I took, that no supposed Germs might either be convey'd through the Air or the Water, or remain adhering

hering to the Subſtances infus'd; I have often, for theſe Purpoſes, made uſe not only of hot Broth, immediately cloſed up in a Phial, but alſo of pure animal Subſtances, ſuch as Urine, Blood, *&c.* with the ſame Succeſs; and in theſe, I believe, no one will ſuppoſe that Germs, Eggs, or Spawn, are pre contain'd, if Care is taken to cloſe the Phials immediately.

Nay I have done more; I have, by reaſoning conſequently to my Principles, been directed to the Choice of many Experiments, all which I conſtantly found to anſwer my Expectation: I have thought, for inſtance, that the more exalted an animal Subſtance was, by a certain Degree of Decompoſition, the more apt would it be to vegetate in a proper Matrix, and form the Part of a larger Animal; or, if it extravaſated, to vegetate into the leſſer; conſequently, that if I took the milky Juice of germing Seeds, or that thick turbid Matter which forms the Wing of a Butterfly in its chryſalidal State, theſe Matters muſt be more exalted than any ordinary Subſtances, and therefore give me theſe microſcopical Productions ſo much the ſooner: And in fact, I never, in theſe Caſes, fail'd of ſeeing them within the Space of a few Hours, while ordinary Infuſions did not give them under ſeveral Days.

Here it will be proper to obſerve, that Naturaliſts have thought the Butterfly's Wing pre-exiſtent in the Caterpiller, becauſe they diſcover'd the firſt Rudiments of it three or four Days before it enter'd the chryſalidal State; but it is then preciſely that the Caterpiller firſt leaves off eating, tho' before extremely voracious; and that probably upon account

count of the Revolution it finds in all its Parts, while its Forces are otherwise employ'd, and the Collection of vegetating ductile Matter it had acquir'd by plentiful Diet, now as plentifully exuberates to form the Parts of the Butterfly. These Truths I am the better acquainted with, because I have particularly examin'd all those Substances: You cannot tear off a Portion of the Butterfly's Wing, even while in the Chrysalid, but you will find it in an Embryo-State, and the Matter which extravasates upon your Object-Glass, if mixt with a little Water to preserve its Fluidity, will almost immediately vegetate into these microscopical Productions. This argues an extreme Activity in it; from Activity follows Action, and an Effect, which can be no other than the Formation of the Wing it was contain'd in.

§ 34. Without instancing in many other Examples, where, by reasoning from these Principles, I was invariably conducted to certain Consequences, this last sufficiently leads to the Nature of animal or vegetable *Semen*. These latter are Substances of the same sort, but more exalted, and from thence adapted to a prompter Vegetation. Of this kind also, but not so exalted, was the gelatinous Substance I obtain'd by common Infusions.

The Exaltation however of Matter does not stop here; the lower I pursued this new Class of Beings in its Descent, the less was this vegetating Force clogg'd with resisting Matter, the swifter was the Motion of the Bodies, and the higher the Degree of Exaltation that produc'd them. This inclines me to believe, that an animal Substance may be exalted this

this way into a Poiſon, a Venom, or a contagious Vapour. Hence ſtagnating Waters are poiſonous and detrimental; and hence perhaps the vipereal Venom, or any other, may derive its Force; for theſe undoubtedly are all animal Secretions. Hence perhaps alſo ariſe contagious epidemical Diſtempers, from a Leaven thrown into the Blood by Exhalations of this kind. I am the more perſuaded of the Truth of this, from the Conſideration of Dr. *Mead*'s Obſervations upon the Venom of the Viper: And ſwift moving Bodies, which ſubſide and ſhoot into Filaments, ſeem manifeſtly to imply all theſe Conſequences. I had myſelf propos'd laſt Summer to try the Effects of ſome of my moſt exalted Infuſions, by inſtilling them into the Veins of Animals; but as yet I have had no Opportunities for theſe Experiments.

I might add other plauſible Conjectures, that ſeem to be the natural Conſequences of theſe Diſcoveries, relating to the Origin of *Aſcarides*, *Teniæ*, *Agaricks*, &c. nay, perhaps I could maintain them with Arguments that would ſeem convincing to moſt Naturaliſts; I might even further ſuppoſe, with ſome Probability, that the muſcular Force, which acts againſt the interſtitial Air in my Friend Dr. *Parſons*'s moſt ingenious Syſtem, in one Word, that all the mechanical Forces of the Body, and the Impreſſions which affect the Soul, may be derived from, and aſcribed to, this vegetating active Force when confined: But I am tired with extending my Views ſo far, nor do I at preſent ſee an End of the Conſequences; the Subject and Principles appear ſo boundleſs.

§ 35.

§ 35. I ſhall conclude therefore with ſumming up my Syſtem in a few Words: I ſuppoſe all *Semen* of any kind to be an exalted Portion of animal or vegetable Matter, ſecreted from the Aliment of every generating Subject, when it is adult, and no further Demand is made for its Increaſe and Growth; this I ſuppoſe to be endued with a proportionable vegetative Force; to be various in various Circumſtances, and heterogeneous in different Subjects; but to be uniform in its Productions, when it falls into a proper *Matrix*, where it finds Matter to aſſimilate, of a Quality and in a Quantity ſufficient to form that ſpecific Being; whilſt in other Circumſtances, it will, if it extravaſates, by the ſame vegetating Force, yield all the ſeveral *Phænomena* I have above taken notice of. And thus, if I am not miſtaken, I have obtained what I firſt intended to make out, that the ſpermatic Animals are not the efficient Cauſe of Generation, but only a neceſſary Conſequence of Principles in the *Semen*, which Principles are neceſſary to Generation.

Thus have I connected my Syſtem with our Countryman Dr. *Harvey*'s Obſervation of that fine Tiſſue, or Web-like Expanſion, obſerv'd in the *Uterus* of Does, in the Center of which the Embryo *Fœtus*, inveſted with its *Amnion* and *Chorion*, was found to be lodg'd: For let the Vegetation begin from the *Semen*, and continue to aſſimilate the affluent Matter from the *Matrix* wherein it has taken Root, and the Fawn muſt come forth like any other ſpecific Animal or Plant.

I ſhall only obſerve, that *Lewenhoeck* had diſcover'd this vegetating Power in the *Semen*, and had,

like Mr. *de Buffon* and me, ſeen the Filaments from whence the ſpermatic Animals ſpring; he even calls them Nerves and Arteries; and in one of his Letters to Mr. *Oldenburg* ſays, that he ſaw more in one Minute than the moſt accurate Anatomiſt could diſcover by Diſſection in a Day: But when he afterwards chang'd this Syſtem, falſe as it was, of Nerves and Arteries for another, I believe, as falſe, that of pre-exiſting Germs in the ſpermatic Animals, he neglected to improve this Obſervation as he might have done; nay he afterwards took no farther Notice of it, but barely to ſay, that it was to be neglected. This Remark I had from Mr. *de Buffon.*

The Difference therefore betwixt Mr. *Lewenhoeck* and Dr. *Harvey* was, that the firſt had an Hypotheſis to maintain, and the latter nothing in View but to follow Nature, without truſting too much to the firſt *Phænomena,* as I hope I ſhall appear to have done in this my Enquiry.

I had almoſt forget one Remark that coincides with my Syſtem; that although animal and vegetable Subſtances by a chymical Analyſis appear to differ, they are nevertheleſs found by a natural Corruption to be reducible to the ſame Principles. This has been obſerved long ago by many Naturaliſts.

And now I think I have nothing more to add, only that I would be underſtood, when I ſpeak of a productive Force in Nature, *&c.* to mean only a Force, which, tho' modell'd by the SUPREME CREATOR, goes no further than the mechanical and material Parts of a Man. I well know that we are compoſed of two very different Principles; and no one mere philoſophical Truth whatſoever preſents itſelf

itſelf to me with more Evidence or Conviction than the Spirituality of our immortal Soul. All have ever allow'd Man in his Origin to be a kind of Plant or Vegetable before he is animated; and all rational Men have deriv'd his Animation immediately from the Fountain of Life, the true Source of all ſpiritual Subſtances. I think I have ſaid no more; and thus only would be taken and explain'd.

The Principle of Life in other Animals I do not examine into, nor do I think it neceſſary. If they are truly ſpontaneous, as they ſeem to be, they have certainly ſome Principle diſtinct from Matter, which the GREAT CREATOR knows when and how to unite.

This Expoſition, Sir, of my Sentiments, I thought might be neceſſary; not that I imagined that either you, or any of the Gentlemen of the learned *Society* in which you preſide, would think my Principles any way tending to Materialiſm, from which no one can be more diſtant or averſe than myſelf; for I well knew that I had nothing to apprehend from Perſons of ſo much Judgment and Diſcernment, and who could not but clearly ſee, that there is really no Connection between thoſe Principles, rightly explain'd, and the Doctrine of the Materialiſts: But I was willing to guard againſt the Miſapprehenſion of others leſs acquainted with Matters of this ſort, and into whoſe Hands this Paper might come, and have therefore taken theſe Precautions.

And now, Sir, I take this Occaſion of returning my moſt humble Thanks both to yourſelf, and to the reſt of the Gentlemen of the *Royal Society*, for the Honour I have received, in being elected one of its Members, and for which I have not been

able as yet to make my perſonal Acknowledgments I hope both you and they will accept theſe Thoughts favourably, which are humbly ſubmitted to impartial Inquiry by the Author, who is, with the utmoſt Eſteem and Reſpect,

SIR,

Your obliged humble Servant,

Turbervill Needham.

Explanation of the Figures in TAB. V.

Fig. 1. Repreſents the Origin of the ſpermatic Animals.

Fig. 2. The Wheat-Infuſion.

Fig. 3. What I have called an Iſland in the Wheat-Infuſion.

Fig. 4. A Groupe of the Chryſalids of the Paſte-Eels.

Fig. 5. Is a Draught of one of the firſt microſcopical Plants or Zoophytes which I diſcover'd; wherein *A* ſhews the Figure of the Plant throwing out its Animals, and *B* the ſame again after the Animals were diſcharged, again putting out a new Shoot from the Stem below, through the hollow tranſparent Head, to form a new Head, and produce another Generation.

VII. Obſ.

VII. Obſervationes aſtronomicæ *variæ factæ in* Paraquaria, *Regione* Americæ Auſtralis, *ab anno* 1706 *ad annum* 1730. *quas cum* Regali Societati *communicavit* Jacobus de Caſtro Sarmento *M. D.* Coll. Lond. Lic. & R. S. S.

Preſented Jan. 28. 1747-8.

ECLIPSES *Solis et Lunæ obſervatæ in Miſſionibus* Paraquariæ, Soc. Jeſu *a* P. Bonaventura Suarez *ejuſdem Societatis Miſſionario, adhibito Teleſcopio quinque Pedum, et oſcillatorio minuta ſecunda exhibente, motu æquali, et per altitudinem Fixarum ad Tempus verum rectificato.*

Eclipſis Solis, anno 1706, *Nov.* 5. in oppido *Sancti Ignatii* ad *Paraquariam*, cujus altitudo poli auſtr. eſt 26°, 52′, ejuſque differentia meridiana ab obſerv. regio *Pariſienſi* horar. 3. min. 57. ſec. 50.

		h.	′	Stylo civili
Initium eclipſeos		8	52	Manè ante merid.
Digiti obſcurati	2	9	15	
	$3\frac{1}{2}$	9	40	
	4	10	0	
	1	11	5	
Finis		11	15	

Maxima quantitas ad hor. 9. m. 50. dig. 4, 0′.

Eclipſis Solis ibidem obſervata anno 1709, *Martii* 11, Stylo civili.

Initium infra horizontem: ortus ſolis ibi hor. 5. 53′.

Digiti

			h	'	Manè ante merid.
Digiti obſcurati	9	20	6	15	
	6	30	6	50	
	6	0	6	54	
	5	0	7	3	
	3	30	7	13	
	3	0	7	17	
	3	30	7	21	
	1	30	7	28	
Finis eclipſeos			7	37	15″

Oculari lente tubo fumo infecta.

Fuit ejus maxima obſcuratio digit. 9 26.

Eclipſis Lunæ ibidem obſervata anno 1707, *Aprilis* 16, poſt meridiem.

	h	'
Initium	7	55
Totalis obſcuratio	8	58
Initium emerſionis	10	45

Finis non eſt obſervatus ob nubes.

Eclipſis Lunæ ibidem obſervata anno 1708, *April.* 4, poſt meridiem.

Immerſio Lunæ	h	'	″	*Emerſio Lunæ.*	h	'	″
In penumb. ſenſib.	12	18	0	Ariſtarch.	14	13	15
In umbram	12	30	29	Plato	14	45	0
Ariſtarch. obſc.	12	37	11	Ex umbr.	15	3	0
Plato obſcur.	12	46	0	Ex pen.	15	12	0

Sereno cœlo.

Eclipſis

Eclipsis Solis ibidem observata anno 1730, *Jan.* 18, post meridiem.

	h	′	″	*Digiti obscurati.*	
Initium	2	52	30	0	0
	2	58	10	1	0
	3	5	0	2	0
	3	19	45	4	15
	3	29	20	5	45
	3	21	22	6	0
	3	39	17	7	0
	3	41	55	7	20
	3	45	0	7	40
	Nubes.				
	4	7	33	8	0
	4	9	36	7	45
	4	11	34	7	30
	Nubes.				
	4	31	0	4	0
	4	42	0	2	0
	4	50	0	0	30

Finis non est observatus ob nubes; videtur fuisse hor. 4, 52′, fere hora 4, 55′; discus solis integer visus; nec luna apparebat in ejus limbo.

Maxima obscuratio videtur fuisse digit. 8 $\frac{1}{3}$.

Anno 1729, nubilo coelo *Augusti* 8, in eclipsi totali Lunæ post mer. hæc tantum observavi, in oppido *S. Ignatii* ad *Paraquar.*

	h	′	″
Initium emersionis	10	1	0
Digiti obscurati 11	10	6	28
Digiti 6	10	33	2

Eodem

Eodem anno 1729, ibidem *Dec.* 9, post meridiem,

h	'	''		
11	3	5	occultavit Luna satellitem ♃	In eodem oppido *S. Ignatii*.
11	13	25	præstrinxit Luna limbum ♃	
11	15	0	occultavit Luna totum ♃	

Eclipsis Lunæ observata in oppido *S. Joseph*, anno 1713, *Dec.* 1, p. merid. Differentia meridiana ad Observ. Reg. *Paris.* hor. 3, min. 52, sec. 30.

	h	'	''
Initium	10	33	31
Finis	12	56	57

Maxima quantitas obscurata dig. 5, fere ad hor. 11, 45'.

Eclipsis Lunæ observata anno 1717, *Martii* 26, p. merid. in ipso meridiano *S. Cosmæ*. Differentia meridiana a *Paris.* $3^h\ 52'\ 20''$. Sereno et tranquillo cœlo.

		h	'	''
Penumbra sensibilis		9	40	0
Initium eclipsis		10	2	21
Digiti obscurati	1	10	8	30
	2	10	15	2
	3	10	13	41
	4	10	31	32
	5	10	40	56
	6	10	52	8
	7	11	10	40

Ope reticuli maxima quantitas obscurata videbatur digitorum 7, min. 18.

Emersio

Emersio Lunæ ex umbra.

		h	′	″
Digiti obscur.	6	11	45	40
	5	12	6	25
	4	12	16	35
	3	12	24	10
	2	12	32	46
	1	12	39	25
Finis Eclipsis		12	45	40
Emersio ex penumbra		13	1	0

Eclipsis Lunæ observata in oppido *S. Michaëlis Archangeli* anno 1728, *Februarii* 24 post merid. tubo 10 ped.

Differentia temporis inter oppid. *S. Mich.* et Obs. Reg. *Parisinum* 3h 48′ 50″.

	h	′	″
Initium eclipsis	14	3	35
Finis eclipsis	17	0	37

Digiti obscurati ad med. ecl. dig. 9. m. 40.

Anno 1700. *Martii* 4 post mer. nondum sacerdotio initiatus observavi rudi Minerva eclipsim totalem Lunæ in Collegio *Fluentino*, vulgo *de las Corrientes*, cujus differentia meridiana inter *Parisios* est 4 2′ circiter.

	h	′
Initium eclip.	13	14
Immersio totalis	14	34
Initium emersionis	16	15
Finis eclipsis	17	15

Anno 1729, *Dec.* 21, p. m.

	h ′ ″	Tubis
Emersio Satel. prim. observ. in *S. Ignat.* ad *Paraquar.*	10 52 49	13 ped.
A clariss. D. *Nic. del' Isle* observata *Petropoli*	16 42 36	15 ped.
Diff.	5 49 47	

Anno 1730, *Martii* 27.

	h ′ ″	
Immersio Satel. 4 in *S. Ign.*	7 23 0	18 ped.
Petropoli	13 12 31	13 ped.
Diff.	5 49 31	

Anno 1730, *Aprilis* 8, p. m.

	h ′ ″	
Emers. Sat. 2. in *S. Ign.*	6 36 45	tubo 13 ped.
Petropoli	12 26 15	tubo 13 ped.
Diff. meridiana	5 49 30	

Alia Satellitum *Jovis* Phænomena, observata in oppido *S. Ignatii* ad *Paraquariam*, p. m.

Anno

1729, *Dec.* 29, 14^h 21′, fuit conjunctio Primi cum Secundo: utraque stella videbatur una.

1730, *Jan.* 23, 9^h 10′, fuit conjunctio Primi et Secundi.

Jan. 25, 15^h 21′ 15″ Primus, et Secundus erant conjuncti, adeo ut uterque videretur unus. 15^h 27′, adhuc visebatur unus: hora vero 15, 36′ erant disjuncti.

1730,

1730, *Mar.* 9, 11^h 36', fuit conj° Sec. et Quarti.
Martii 12, 10^h 9' fuit occultatio Secundi retrogradi in margine ♃.
Martii 18, 6^h 38' fuit conjunc. Sec. et Tertii.
Martii 29, 9^h 7' 40'' fuit occultatio Tertii directi in margine ♃.
Martii 30, fuit occultatio Secundi Retr. in limbo *Jovis*, 7^h 21' 30''.
Martii 31, fuit occultatio Primi Retr. in limbo *Jovis*, 9^h 21' 15''.
Aprilis 1, 6^h 36' 25'', tub. ped. 18, observavi occultationem Primi Directi in margine *Jovis*: hora verò 10, 16' 57'' emersit ex umbra *Jovis*.

1729, *Dec.* 9, 11^h 3' 5'', p. m. præstrinxit margo Lunæ Satellitem tertium *Jovis*. Initium occultationis *Jovis* fuit 11^h 13' 15''. Occultatio totalis *Jovis* in margine Lunæ fuit 11^h 15'.

1730, *Aprilis* 27, apparebant *Saturni* ansulæ valde exiles; sed *Maii* 8, 17^h, erat *Saturnus* rotundus, et suis ansulis penitus orbatus.

Observationes astronom. a *P. Bonaventura Suarez*, in Miss. *Paraquariæ*, Soc. Jesu, in oppido *S. Ignatii* ad *Paraquariam*. Est aliud oppidum *S. Ignatii* orientalius ad flumen vulgo *Uruguay* nuncupatum.

Oppidum *S. Ignatii* (reliquis occidentalius) distat a civitate *Assumptionis Paraquariæ* versus austrum 50 leucis *Hispanis*.

Latitudo *Assumptionis* a me observata gr. 25, m. 14, Austr.

Latitudo *S. Ignatii* gr. 26, m. 52.

8 * 2 Diff.

		h	′	″	
Diff. mer. *S. Ignat.*	a *Petropoli*	5	59	40	Omnes occiden.
	a *Parisiis*	3	57	50	
	a *Londino*	3	48	40	
	ab opp. *S. Cosmæ*	0	5	30	

Emersiones Satell. Primi, p. m.

Anno.		d	h	′	″	
1729	*Januarii*	26	8	16	42	tubo ped. 13
	Februarii	18	8	28	13	13
	Martii	6	6	50	18	13
	Martii	13	8	47	4	13
	Immersio Primi.					
	Novembris	3	16	20	36	13
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